JOURNAL

OF THE

FRANKLIN INSTITUTE

OF THE STATE OF PENNSYLVANIA,

FOR THE

PROMOTION OF THE MECHANIC ARTS.

VOL. CIV. OCTOBER, 1877. No. 4.

THE Franklin Institute is not responsible, as a body, for the statements and opinions advanced by contributors to the JOURNAL.

Franklin Institute.

HALL OF THE INSTITUTE, Sept. 19th, 1877.

The stated meeting was called to order at 8 o'clock P. M., the President, Dr. R. E. Rogers, in the chair.

There were present 185 members and 36 visitors.

The minutes of the last meeting (June 20th, 1877) were read and approved.

The Actuary presented the minutes of the Board of Managers, and reported that at the meeting held June 15th, last, a protest was entered against the proposed award of the Elliott Cresson Gold Medal to John Charleton, for his internal clamp coupling, for alleged want of originality, and that the question was referred back to the Committee on Science and the Arts, for report, where it is still pending.

The Actuary further reported that at the meeting of the Board held July 1st, the following resolution was adopted:

Resolved, That the Board of Managers recommend to the Institute the election of Mr. Frederick Ransome as an honorary member of the Institute.

Also, that since the meeting in June last, 20 persons have been elected members of the Institute; and at the last meeting the following donations to the Library were reported:

Sixth report upon the improvement of the South Pass of the Mississippi River, showing the condition of the work March 16th, 1877, by C. B. Comstock. From the Chief of Engineers.

Specifications and Drawings of U. S. patents, for February, 1877. From the Patent Office.

Die pflanzenwelt Norwegens, by Dr. F. C. Schübeler. Christiania, 1875.

Anden beretning om Ladegaardsoens hovedgaard. Andet hefte. Christiania, 1875. From the University of Christiania.

Memoirs of the geological survey of India, Vol. 12, Pts. 1 and 2. Palæontologia Indica, Ser. X, 2, and X 1, 1. Also Records. From Geological Survey of India.

Railroad Dom Pedro II, Department of traffic. Report for 1876. From Engineer F. P. Passos.

Report on the Mississippi jetties, by Jas. B. Eads, Chief Engineer, Aug. 18th, 1876. From G. Leverick.

Treatise on the rationale of compressing cotton, by S. H. Gilman. 2d Ed. New Orleans, 1877. From the Author.

New Encyclopædia of Chemistry, Pts. 21 to 25. From J. B. Lippincott & Co., Pubs.

Economic theory of the Location of Railways, by A. M. Wellington, C. E. From the Railroad Gazette, N. Y.

A Duplex partial earth test, by W. E. Agrton and John Perry.
Preliminary catalogue of the apparatus in the telegraph museum, by W. E. Agrton. 1877.
The importance of a general system of simultaneous observations of atmospheric electricity, by W. E. Agrton and John Perry. Yokohama, 1877.
The specific inductive capacity of gases, by John Perry and W. E. Agrton. Yokohama, 1877.
From the Imperial College of Engineering, Tokio, Japan.

The Secretary presented his Report, which embraced an illustrative description of the following subjects: Hancock's Inspirator, or Double Injector, for feeding steam boilers; J. G. Baker's Rotary

Circulating Pump (being a modification of his rotary pressure blower); the Articulating Telephone, invented by Prof. A. Graham Bell; and Brush's Magneto-Electric Machine.

An interesting discussion, participated in by a number of members, followed the presentation of the telephone. During the discussion the President called Vice-President Close to the chair, and the latter presided during the remainder of the meeting.

On motion of Mr. W. P. Tatham, seconded by the President, Dr. Rogers, Mr. Frederick Ransome, of London, was unanimously elected an honorary member of the Institute.

On motion, the meeting adjourned.

J. B. KNIGHT, *Secretary.*

BELL'S ARTICULATING TELEPHONE.

Extract from the Secretary's Report, at the Meeting of the Franklin Institute, Sept. 19th, 1877.

The principle upon which the articulating telephone primarily depends for its action, was among the discoveries of Faraday—that any disturbance of the magnetic condition of the core of an electro-magnet induces a current of electricity in the coil. It is also known that if a piece of iron be made to approach or recede from the pole of a permanent magnet, the magnetic condition of the latter will be changed. If, now, we use the pole of a bar-magnet as the core of a coil of insulated wire, we have but to make a piece of iron approach and recede from the pole of the magnet to produce a current of electricity in the coil.

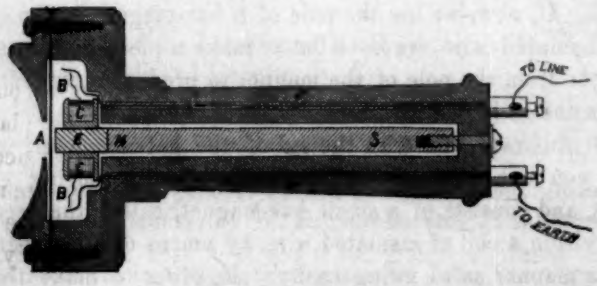
I will illustrate this with the aid of the galvanometer, which, as most of you are aware, is an instrument for measuring electrical currents, and consists of a small bar-magnet, called the needle, suspended within a coil of insulated wire, by means of a filament of silk, in such a manner as to swing easily. In order to make the movements of the needle apparent to the audience, there is attached to it a small mirror, from which is reflected that small spot of light on the screen.

The principle on which the galvanometer works, is that if a current of electricity is passed through the coil, the needle will tend to place

itself at right angles to the wire in the coil. As it is now placed, the needle is parallel with the wire. If, therefore, I place one pole of this bar-magnet within this coil of insulated wire, and connect the ends of the latter with the ends of the wire in the coil of the galvanometer, and approach this piece of iron near to the pole of the magnet, the needle of the galvanometer should be deflected in proportion to the strength of the current induced in the coil surrounding the magnet, and you should see the spot of light move toward one side of the screen. I now do this, and, you observe, that as I place this piece of metal quickly near to the magnet, the spot of light moves toward your left; and as I withdraw the iron quickly, the spot of light moves in the opposite direction, showing that the current is reversed.

All these facts were known before Prof. Bell commenced his investigations, but it remained for him to make the discovery—which enabled him to utilize these laws in the transmission of articulate speech—that if the piece of metal, intended to change the magnetic condition of the core of the coil, was made into a thin sheet and placed in front of it, and two such instruments were connected by a line of wire attached to one end of each coil, and the other ends connected to the ground, that sounds made in front of one of these plates would be reproduced by the other.

The manner in which he has utilized these laws is shown in this illustration, which is a cross-section of the hand instrument.



Inside of the wooden case *DD*, is placed the bar-magnet *NS*, having attached to one of its poles a piece of soft iron *E*, which becomes charged with magnetism of the same sign as the pole to which it is connected, and thus, really, forming part of the magnet. Around this piece of soft iron as a core, is wound the coil, consisting of about

60 ft. of No. 38 insulated copper wire, the ends being carried to the binding screws. A diaphragm, consisting of a circular plate of very thin iron, is placed at right angles to, and its centre opposite, the axis of the magnet, and as near it as possible not to touch when vibrating. This diaphragm is held firmly at its edge between the body of the case and the cap, by means of the wood screws, as shown.

By placing the instrument near the mouth while speaking, the diaphragm is made to vibrate by, and in accordance with, the sound-waves in the air, producing induced currents in the coil, which rapidly change in intensity and direction. When two telephones are properly connected by a line of wire, and to the ground, the electrical currents produced in the first instrument are conducted through the coil of the second one, and produce an increasing and diminishing attractive force in the magnet, and thus reproducing the vibrations of the first diaphragm, and consequently setting up the same sound-waves in the air near it. By placing the second instrument to the ear, the voice is heard in every intonation and quality, so that one is able to recognize the speaker by his voice.

This telephone, and two others in the body of the room, are connected, through about 4000 feet of wire, to similar instruments at No. 1111 Chestnut St. I will now signal to the person in attendance there, that we are ready, by means of this magneto-electric apparatus, and you will be able to hear the conversation carried on between that point and this room.[1]

The number of instruments in the circuit seems to make very little difference in their action; we had six or seven on the line yesterday, and apparently could hear as well as with only two. You will observe that the sounds conveyed can only be heard by placing the instrument to the ear, and therefore that the newspaper accounts of the speaking telephones being heard by a whole audience are mistakes.

The extreme distance through which this telephone can be used has not been definitely stated, but it has been used successfully between

[1] This was done, and conversation was carried on with almost the same ease as if the person talking were personally present. Several instruments were placed in the circuit, in different parts of the room, and each one conveyed the sound equally well. After some experiments in speaking, the telephone on Chestnut Street was placed on the sounding-board of a piano while being played, and the tune was heard quite distinctly by those listening at the Institute.

points 32 miles apart. We made some experiments to-day with a rheostat, and spoke through a resistance equal to 550 miles of ordinary telegraph wire, stretched on poles. While theoretically the instrument is capable of being used on a wire of that length, practically it could not be so used, partly on account of the interference of induced currents from other wires, but principally because of what is termed escape or leakage, by which the current would be dissipated.

Great care is necessary in erecting lines for this telephone, for if the wire comes in close proximity to, or is carried on the same poles as, other lines, other currents are induced, producing confusion.

Part of the line we are now working on is carried on the poles of the city lines, and you can hear distinctly the click of the various Morse instruments operating them.

This telephone, the invention of Prof. A. Graham Bell, of Boston, should not be confounded with that of Elisha Gray, of Chicago, Ill., or of T. A. Edison, of Newark, N. J., both of which have been used in public for conveying musical sounds.

The two distinguishing features of the Bell telephone are that it is the only one not using a battery, and that conveys articulate speech by means of electrical currents.

The Bell telephone is now in practical use in New York and Boston, and several lines are in course of construction in this city.

Electric Light for Cities.—The municipal council of Exeter is the first public body in England which has officially recognized the value of the recent experiments in electric lighting. It has postponed a contemplated purchase of gas-works, under a hope that electricity will soon be shown to be more satisfactory as well as more economical. The three rival systems, of the Gramme machine, the Alliance machine, and the electric candles of Jablochkoff and Denayrouze, are continuing their experiments in Paris, on a large scale.—*Nature; Les Mondes.* C.

Compressibility of Liquids.—E. A. Amagat has conducted a series of investigations on the compressibility of volatile liquids, when the liquid state was maintained by pressure, at a temperature higher than their boiling point. The experiments were carried to a pressure as great as 39 atmospheres, the results being in the most satisfactory accordance with deductions from the mechanical theory of heat.—*Comptes Rendus.* C.

HISTORICAL SKETCH OF SHORTHAND IN PHILADELPHIA.[1]

The first shorthand writer in Philadelphia, of whom we have any knowledge, was Thomas Lloyd, who reported the debates in the first Congress of the United States, and was legal reporter in this city for many years. During this time, he returned to his native land (England), and published some of the debates, for which he was thrown into Newgate as a political prisoner, and remained there five years. He was the author of a system of shorthand, which he taught here. Reports of debates in the House of Representatives, taken by him in 1789, are extant; as also, reports of trials taken in 1820.

Later, M. T. C. Gould practiced and taught a system of shorthand, in this city.

In the year 1848, Oliver Dyer introduced Pitman's phonography, and taught a class in the Boys' High School. The influence of his teaching was soon felt, and the Philadelphia Phonographic Society was organized April 12th of that year. Among the members of that society, was Townsend Sharpless, who was an enthusiastic advocate of introducing phonography into the public schools, and of its use in business houses: an idea now practically realized in England; also, Franklin Peale, a noted mechanician of Philadelphia, and for many years Chief Coiner of the United States Mint; Clinton Gillingham; Oliver Dyer; and Robert Paterson, who was the author of the first book on Phonographic Reporting ever published in this country.

Many of the members of that society have been prominent advocates of phonography ever since, and are well known to phonographers throughout the United States. In addition to those above mentioned, are Professor Booth, the author of "Booth's Phonographic Instructor," and Prof. James A. Kirkpatrick, who taught the system for many years in the High School, and is now an officer of the Phonetic Shorthand Section of the Franklin Institute. The efforts of the members of that society appear to have been to perfect themselves in the art, and there is no record of any attempt to go further than that. The last meeting was held June 6, 1849.

The society had the honor of numbering in its list of members, Dennis Murphy, than whom there is no better phonographer, and

---

[1] Translation of the phonography printed in audatype on page 225.

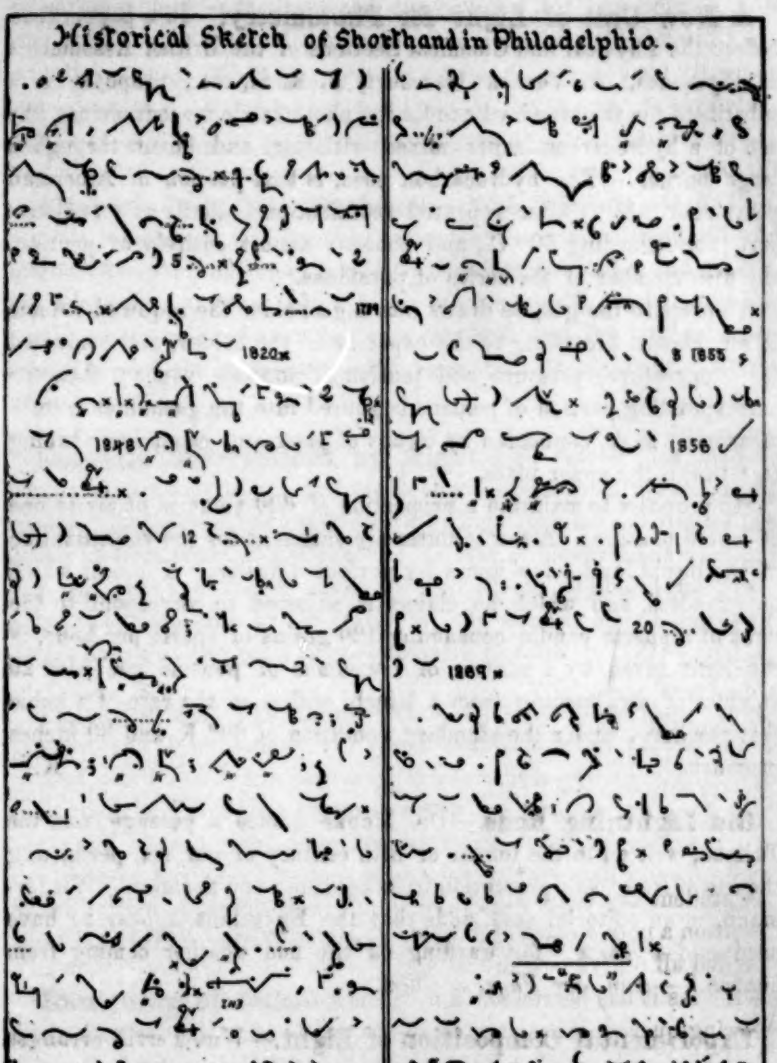
who is now, and for many years has been, the chief reporter in the United States Senate; and John McElhone, who has been permanently connected with the reportorial staff of the House of Representatives of the United States for nearly an equal length of time. These gentlemen commenced the study of phonography at the High School, under Mr. Dyer, which fact alone is a strong argument in favor of the utility of introducing phonography, as a regular study, into the public schools.

No other organized society existed up to February 8th, 1855, when the Philadelphia Phonographic Society was revived. Through the efforts of this society, phonography was introduced into the Girls' Normal School in the spring of 1856, where it was taught by D. Shepherd Holman, until the regular teachers of the school were competent to continue the instruction. The study was thrown out of this school at the close of the year, the Board of Controllers considering that the pupils were overcrowded with studies. Phonography was taught in the High School for over 20 years, being abandoned there in 1869.

The present society, it is hoped, will be able to demonstrate that the public schools are the proper places to commence the study of this important branch of education, which is of immediate value, not only in business pursuits, but also for the aid it is capable of constantly affording the pupils in the prosecution of their studies. As a Section of the Franklin Institute, it is believed its influence will be more sensibly felt and more enduring than that of the organizations which preceded it.

A student of the so-called "standard phonography," read without hesitation a note written in Pitman's phonography. Pitman's system preceded all others now in use, and is more widely practiced than any other. As it has borrowed from none of the others, we may put the following query: Whose system is entitled to be called "standard?"

The Franklin Institute class in phonography will commence on Monday, October 8th, 1877.


Historical Sketch of Shorthand in Philadelphia.

A New Unit of Light for Photometry.—In a paper read before the Physical and Chemical Sections of the British Association at Plymouth,[1] A. Vernon Harcourt, F. R. S., etc., proposes as a substitute for the standard candle for photometric measurements, the use of a hydrocarbon vapor mixed with air, and burnt through a large burner. The hydrocarbon used, is that portion of American petroleum, which, after repeated rectifications, distils at a temperature not exceeding 50° C., and consists almost entirely of pentine, the fifth member of the series of paraffines.

To prepare the gas, he draws into a gasholder the required volume of air, chosen according to the capacity of the holder, and corrected for temperature, pressure and tension of aqueous vapor; then the corresponding portion of pentine is poured into the gasholder from a measuring flask, connected by means of glass and caoutchouc tubing to a tap in the upper plate.

He proposes to maintain a proportion of 600 volumes of air to one of liquid pentine. A few minutes are sufficient for the volatilization of the liquid, and a few hours for perfect diffusion. The unit which he proposes, and which he claims is adjusted to correspond to the light of a sperm candle consuming 120 grains of sperm per hour, is the light given by a mixture of 7 volumes of pentine gas with 20 volumes of air, burning from a ¼-inch orifice at the rate of a cubic foot per hour, under the standard condition of 60° F. and 30 inches pressure. K.

Old Lightning Rods.—Dr. Munke quotes a passage from the Talmud, written in the fourth or fifth century of our era, permitting the use of iron "as a protection from lightning and thunder." Wiedermann, in an editorial note, adds that the Egyptians appear to have used gilded masts "for warding off the bad weather coming from heaven."—*Ann. der Phys. u. Chem.* C.

Experimental Composition of Light.—Wm. Terrill arranges seven lanterns, with glass slides stained to imitate the different colors of the spectrum. By turning the lanterns so that the projected circles overlap, a circle of white light is produced. Interesting experiments with complementary colors may be performed in the same way.—*Nature.* C.

[1] See *Chemical News*, Sept 7th, 1877.

Electrolytic Experiments.—M. Gramme reports three sets of experiments with his machine on the electrolysis of sulphate of copper. The results show that intensity is much more important than quantity, and that little or no work is expended in the electrolysis of sulphate of copper with copper anodes.—*Comptes Rendus.* C.

Alteneck Induction-Machines.—Prof. Weinhold adds his testimony to that of the Russian experimenters, in favor of the von Hefner-Alteneck inductors, made by Siemens and Halske, in Berlin. He represents them as having the best form for powerful currents, giving most compactly, at the least cost and with the least expenditure of power, the strongest possible currents.—*Bayer. Ind.- u. Gew.-Blatt.* C.

Geodetic Observations by Night.—F. Perrier has made a comparative study of day and night observations, from which he concludes "that azimuthal observations by night possess a degree of precision at least equal, if not superior, to that of observations by day." He will make a special study of the effects produced upon the azimuthal measures by the torsion of the wooden signals under the direct action of the sun's rays.—*Comptes Rendus.* C.

Pulsation and Vibration.—C. A. Bjerknes finds that "simultaneous and synchronous vibrations manifest mean forces of the second, third and fourth degrees; and for these new apparent forces the principle of equality between action and reaction subsists, while they also show a great resemblance to the forces of nature. Thus two spheres, having concordant pulsations, attract each other inversely as the square of the distance; they repel each other according to the same law if their pulsations are opposed."—*Comptes Rendus.* C.

Hardening of Oak.—Among the numerous instances of the hardening of oak under water, Berthier mentions remains of bridge piles, at Rouen, which were driven in 1150. They resembled ebony in texture and color, and the change was attributed to peroxide of iron. M. Charil-Marsaines, in destroying the remains of a dike which was built by Vaubam in 1681, found that the oak timbers had the same appearance. He did not examine them chemically, but he quotes experiments of Buffon, to account for the probable presence of an iron oxide.—*Ann. des Ponts et Ch.* C.

Plastic Flow.—Prof. Friedr. Kick and Asst. Ferd. Polak, of Prague, have experimented upon the internal displacements in soft masses or plastic bodies, under external pressures. Their experiments cover only a small portion of Tresca's field, but they are neatly treated, and formulæ of deformation simply deduced. They conclude that the laws of deformation are the same for all bodies which admit of internal displacements through pressure. Some of their experiments with clay may have important bearings on geological erosion.—*Dingler's Polyt. Jour.* C.

Barometric and Magnetic Rotations.—In discussing the observations of Stonyhurst College, attention was called to a probable connection between the movements of the barometer and those of the magnetic declination (see *Les Mondes*, July 7, 1877). Communications were presented to the American Philosophical Society in 1863 (Proc. A. P. S., ix, 283, 291, etc.), demonstrating such a connection, and the Society awarded its Magellanic premium of a gold medal for the discovery, in 1864.—*Ibid.*, p. 486. C.

A New Method of Organic Synthesis.—C. Friedel and J. M. Crafts have effected many syntheses of complex hydrocarburets and acetones, by means of reactions initiated by metallic chlorides, iodides and bromides. The action of the chlorides is generally the neatest; the organic chlorides are attacked by the metallic chloride, with loss of chlorhydric acid, formed from two molecules, of which one furnishes the chlorine, and the other the hydrogen, and of which the two residues combine. They hope, by these reactions, to obtain new radicals of the fatty and aromatic series.—*Comptes Rendus.* C.

Coupled-Leveling Instrument.—Engineer Aita, being charged with the survey of Padua for drainage and water-service, devised a coupled level which remedies many of the surveying difficulties of tortuous and narrow streets. It consists of two glass tubes, doubly clamped to graduated staffs, and connected by a caoutchouc tube of any desired length. The two clamps are movable, the glass tubes being partly, and the connecting tube wholly, filled with water. The observer at each end brings one of the clamps to the water level, and enters the scale-reading in his note-book. When the books are compared, the difference of readings gives the difference of altitudes.—*Il Politecnico.* C.

British Encouragement to Science.—An influential deputation of members of Parliament lately waited on the Chancellor of the Exchequer, to advocate the claims of the Scottish Meteorological Society. Sir Stafford Northcote said that the Treasury was prepared to grant £1000 for services rendered to the Government during the past twenty years; and he gave encouragement for future appropriations.—*Nature.* C.

Measurement of Water Suspended in Steam.—P. Gazzi, in view of the difficulties introduced into calculations of boiler efficiency by foaming or other mechanical suspension of vapor, has described an apparatus for determining the degree of humidity, as well as for finding the density, either of saturated or of dry steam, at high pressures. His invention was suggested, in part, by the recent investigations of Hirn, Leloutre, and Hallauer.—*Il Politecnico.* C.

Influence of Mechanical Action on Crystallization.—Supersaturated solutions of many salts, often spontaneously deposit crystals which are less hydrated than the common salt. In many cases, before the point of spontaneous deposit is reached, a crystal of the less hydrated form will start the deposit. In such cases a brisk friction against the walls of the vessel will often produce crystallization. D. Gernez has observed three modes of this mechanical action: 1, the simple production of the least hydrated crystals; 2, the production of the most hydrated crystals, in solutions where the other hydrate would form in contact with a crystal; 3, the production of either salt, under different degrees of friction.—*C. R.* C.

Cooling Goblet.—M. Toselli has invented a simple contrivance for the rapid cooling of liquids. It consists of a cylindrical cup, for holding any beverage, into which may be plunged an inner goblet, shaped like an inverted truncated cone, and having a lid which rests on the outer cup. Putting 150 grammes of nitrate of ammonia in the inner goblet, filling it with cold water, and stirring it so as to hasten the solution, the temperature of the outer liquid is soon reduced at least 12° C. (22° F.). The salt may be used for an indefinite period, by spreading it on a plate after each trial, and exposing it to the sun until it crystallizes anew. The inventor prepares a salt which will lower the temperature 28° C. (50° F.) in the warmest countries.—*Les Mondes.* C.

Hydraulic Engineering in France.—M. de Lesseps has called the attention of the French Academy to the distribution of the waters flowing from the natural declivities of the French territory, and to the improvement of internal navigation. In view of the wealth of its hydrographic basins, of their importance, and of the facility of inter-communication, France is the most favored country of Europe. Far from profiting by this happy position, and notwithstanding the abundance of rivers, the meadows are of a limited extent, large tracts are exposed to droughts which oppose the improvement of their culture, and immense amounts of valuable material are continually wasted. M. Herve Mangon estimates that the volume of mud annually carried off by the Durance, bears to the sea more than 14,000 tons of nitrogen, in the state of combination which is best fitted for the nourishment of cultivated plants. At the same time, agriculturists buy from foreign countries, at great sacrifice, other nitrogenous matters, and the importation of guano, which scarcely furnishes so large a quantity of nitrogen to French agriculture, costs it thirty million francs per annum. The same mud contains nearly 100,000 tons of carbon, or as much as would be furnished by a forest of 50,000 hectares.

M. Cotard proposes to store all the summit-waters, in the basin of the Garonne and the Adour, and distribute them in such manner as to feed the channels of internal navigation, facilitate the transport of industrial products to good markets, and avoid the accumulation of stagnant waters in unhealthy marshes. The French Agricultural Society endorses his project with strong recommendations.

Hubert Delisle sets forth all the benefits of the water-courses in transporting bulky and cheap products, regarding it as an important object to put all parts of the territory into easy communication. He would develop five great lines: 1, the Eastern line, putting Havre and the northern ports in communication with Alsace and Switzerland by the Seine, the enlargement of the sluices of the canal from the Marne to the Rhine, and needful improvements along that canal. 2, the line of the grand-girdle canal, connecting the rich basins of the Oise, the Aisne, the Marne, the Aude, the upper Seine, and the Yonne, thus establishing relations, on one hand with the navigable waters of the north, on the other with the channels of Burgundy and Orleans. 3, the Western line, to put Nantes and the departments watered by the Loire, the Santhe, the Mayenne, the Cher and the Vienne, in communication with Paris, the north, the east and the

centre of France. 4, the southwestern line, joining the basins of the Dordogne and the Garonne to the capital by an artery which would tap the coal basins, including the canal of the great *Landes*, uniting Bayonne and Bordeaux. 5, the line of the Saône and the Rhone, by its natural and artificial affluents, penetrating far into the interior and surrounding the Alps, is capable of serving central France, Switzerland, Belgium, and Western Germany; it has, therefore, in view of the commercial relations of the continent with the Mediterranean, an importance unrivaled by any other river in Europe.

Capt. Sibour shows that the improvement of the navigable ways and the appropriation of the canal and harbor of Bouc, should be completed by opening a canal of 7 kilometres between the pool of Berre and the harbor of Marseilles, permitting an immense development of commerce and industry as well as an indispensable security for vessels. Twelve years ago, Baron Chabaud Latour, Engineering Inspector-General, wrote: "The government should not hesitate to employ the magnificent natural harbor of the port of Berre, and to execute the civil and military works which will make it one of the most powerful centres of commercial activity in the world."

"Water courses," says M. Krantz, "awaken agricultural improvements, encourage the establishment of workshops, facilitate the development of mines, quarries and forests, increase the public wealth, and the State receives its share of the wealth which they create." M. de Lesseps hopes that the scientific concurrence and the united influence of the members of the Academy, will contribute to the realization of their several plans.—*Comptes Rendus.* C.

Iron Railway Ties.—Engineer G. Cattaneo describes the advantages of the Hilf system, its gradual extension and success in Germany, the proposed modifications of Hensinger, and the reasons for its adoption in Italy.—*Il Politecnico.*

Holmes' Distress Signal.—The signal is made by a projectile thrown from a mortar to distances varying from 500 to 2500 metres. On touching the water, it floats on the surface, and immediately blazes, emitting a very white and intense flame for thirty or forty minutes. A half-dozen of these projectiles, shot from a besieged building, would surround it with a line of inextinguishable light, so that the occupants, while in the shade themselves, could examine the torpedoes and other offensive contrivances of the enemy.—*L. M.* C.

Book Notice.

A Treatise on the Use of Belting for the Transmission of Power. Largely illustrated. By John A. Cooper. 8vo, pp. 300. Philadelphia, 1877. Claxton, Remsen & Haffelfinger.

Of all the appliances used by the mechanical engineer, there is none upon which writers disagree so widely as the transmission of power by belting, and therefore a collection of data from actual practice became very desirable. Such a collection was begun some years ago by Mr. Cooper, and published in The Journal of the Franklin Institute as obtained.

Later, Mr. Cooper saw the great advantage of having in one volume all the important literature on the subject, which, up to that time, was so scattered as not to be available, except with great labor to the few having access to the best engineering libraries.

This work has been performed very exhaustively by the author, resulting in the most complete collection of rules, tables and statistics upon the use of belts now in print.

He has given his own rules and formulæ for calculating the width, strength and arrangement of belts for all kinds of service, but being simply a seeker after truth, has not hesitated to place beside them the rules and opinions of others, even when they do not agree with his own, and in every case he has given full credit to those whose opinions he has borrowed.

To make an exhaustive review of this valuable work would be to write a second treatise, but it may be stated that among other valuable matter, the essay of Mr. Robert Briggs, the experiments of Mr. John H. Town, and a full translation of the entire treatise of M. Morin on this subject, are given. There is also much valuable information regarding transmission by endless ropes and by rolling contact.

Exception may be taken to the repetition consequent upon quoting so many original papers in full, but this was unavoidable in carrying out the author's plan of making it a standard work of reference, and as such it makes it all the more valuable.

After all the labor of preparing such a work, Mr. Cooper has not buried it between the covers without a proper guide to its contents, as is sometimes the case with authors, but has given a thorough and complete index, covering ten pages. K.

THE METHOD OF LEAST SQUARES APPLIED TO A HYDRAULIC PROBLEM.

By MANSFIELD MERRIMAN, Ph. D., Instructor in the Sheffield Scientific School, New Haven, Conn.

In the great hydraulic survey of the Mississippi river, conducted by Humphreys and Abbot, numerous velocity observations with double floats, were taken at different depths below the water surface. The combined results of all these observations are given on page 244 of the second edition of their *Report on the Physics and Hydraulics of the Mississippi River*, and are as follows:

Depth of float below surface.	Observed velocity in feet per second.	Remarks.
Surface.	3·1950	"Grand mean of all observations taken from anchored boats, combined in ratio of number of observations at each determined point. They were taken at Carrolton and Baton Rouge, in 1851. Each point is fixed by 222 observations. Mean maximum velocity, which is 0·297 *D* below the surface, is 3·2611 ft. Mean wind is down force 0·2. Mean velocity of river is 3·3814 ft. per second."
0·1 *D*	3·2299	
0·2 *D*	3·2582	
0·3 *D*	3·2611	
0·4 *D*	3·2516	
0·5 *D*	3·2282	
0·6 *D*	3·1807	
0·7 *D*	3·1266	
0·8 *D*	3·0594	
0·9 *D*	2·9759	
Bottom.		

These observations may be graphically represented by dividing a vertical line equal to *D*, the depth of the river, into ten equal parts, through the points of division drawing horizontal lines, and laying off upon these the observed velocities. The points thus obtained will show more clearly than the above figures, the manner in which the velocity varies from the surface to the bottom. In the diagram below the points enclosed within small circles represent these observations. Each horizontal division of the diagram is 0·1 ft. per second, and each vertical division represents one-tenth of *D*.

Now if we can obtain the equation of a curve passing through these points, we shall have not only a formula from which the velocities at all intermediate points may be computed, but also an expression of the law governing the velocities at different depths.

Humphreys and Abbot give reasons for concluding that a parabola, whose axis is horizontal, will better accord with these points than any other curve of the second degree. Their method of deducing the

equation of this parabola was one of trial—the constants of the equation were first virtually assumed, then corrected by comparison with the observations, after which the curve was plotted on tracing paper, and being laid upon the points "the eye at once detects if a close approximation can be made by slightly changing the depth of the axis," or the value of the parameter. This process is fully described on page 243 of the Report (second edition), and by it "the following equation was deduced for the parabola corresponding to the grand mean curve of observations:

$$V = -0{\cdot}79222\, d_{11}^{\,2} + 3{\cdot}2611,$$

in which V is the velocity in feet, and d_{11} is the distance from the axis, in fractional parts of the whole depth, considered unity." The position of the axis is stated as $0{\cdot}297\ D$ below the surface. The last three columns of the following table, which are taken from page 244 of the Report, "exhibit the comparison between the observations and this parabola."

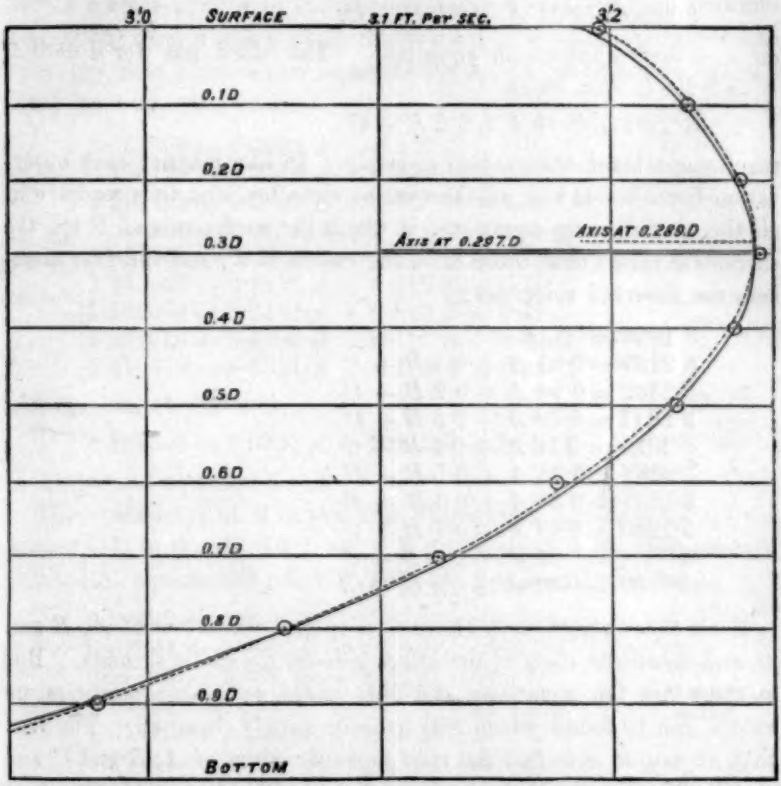
TABLE I.

Depth of float below surface.	Corresponding value of d_{11}.	"Velocity by observation."	"Veloc. by above equation."	"Difference."
		FEET.	FEET.	FEET.
Surface.	+ 0·297	3·1950	3·1901	+ 0·0049
0·1 *D*	+ 0·197	3·2299	3·2293	+ 0·0006
0·2 *D*	+ 0·097	3·2532	3·2525	+ 0·0007
0·3 *D*	— 0·003	3·2611	3·2600	+ 0·0011
0·4 *D*	— 0·103	3·2516	3·2525	— 0·0009
0·5 *D*	— 0·203	3·2282	3·2274	+ 0·0008
0·6 *D*	— 0·303	3·1807	3·1873	— 0·0066
0·7 *D*	— 0·403	3·1266	3·1313	— 0·0047
0·8 *D*	— 0·503	3·0594	3·0596	— 0·0002
0·9 *D*	— 0·603	2·9759	2·9719	+ 0·0040
Bottom.	— 0·703		2·8685	

On the accompanying diagram I have laid off the values in the column headed "Velocity by above equation," and drawn through the points the full line curve. The agreement between this curve and the observations is very close.

The method used for deducing this equation is long and tedious, as anyone may satisfy himself by trial, and moreover very unscientific, since there is no reason to suppose that it gives the curve best agreeing with the observations, and since no two persons following it would arrive at the same result. The Method of Least Squares, however,

furnishes a simple and strictly scientific process by which the exact equation of the curve most closely agreeing with the observations can be determined. In the last number of this JOURNAL I gave a discussion of the mathematical principles at the basis of this Method, and as an example of its use in practice, will now apply it to the deduction of the parabola best agreeing with the above observations.



The general equation of a parabola whose axis is horizontal, is

$$V = Ad^2 + Bd + C.$$

The depth of the river is here taken as unity, and d is any fractional part of that depth measured from the water surface downward; V is the velocity at a point whose depth is d; A, B and C are constants, to be determined from the observations, fixing the dimensions and position of the curve.

The first of the above observations shows for the surface, or $d = 0$, a velocity of 3·1950 feet per second. These values inserted in the above parabolic expression, give

$$3{\cdot}1950 = C$$

as a first *observation equation*. The second has for $d = 0{\cdot}1$, $V = 3{\cdot}2299$, and these inserted, give

$$3{\cdot}2299 = 0{\cdot}01\,A + 0{\cdot}1\,B + C$$

as a second observation equation. The third has for $d = 0{\cdot}2$, $V = 3{\cdot}2532$, from which

$$3{\cdot}2532 = 0{\cdot}04\,A + 0{\cdot}2\,B + C$$

results as a third observation equation. In like manner each observation furnishes us with an observation equation, and thus we have in all the ten following equations, in which the coefficients of B are the successive values of d, those of A the values of d^2, and the first members the observed velocities:

$$\begin{aligned}
3{\cdot}1950 &= + C \\
3{\cdot}2299 &= 0{\cdot}01\,A + 0{\cdot}1\,B + C \\
3{\cdot}2532 &= 0{\cdot}04\,A + 0{\cdot}2\,B + C \\
3{\cdot}2611 &= 0{\cdot}09\,A + 0{\cdot}3\,B + C \\
3{\cdot}2516 &= 0{\cdot}16\,A + 0{\cdot}4\,B + C \\
3{\cdot}2282 &= 0{\cdot}25\,A + 0{\cdot}5\,B + C \\
3{\cdot}1807 &= 0{\cdot}36\,A + 0{\cdot}6\,B + C \\
3{\cdot}1266 &= 0{\cdot}49\,A + 0{\cdot}7\,B + C \\
3{\cdot}0594 &= 0{\cdot}64\,A + 0{\cdot}8\,B + C \\
2{\cdot}9759 &= 0{\cdot}81\,A + 0{\cdot}9\,B + C.
\end{aligned}$$

From these equations we have now to find the values of A, B and C, and substitute them in the above general parabolic formula. But as there are ten equations, and only three unknown quantities, no values can be found which will exactly satisfy them all. The best that we can do is to find the *most probable* values of A, B and C, and these, according to the principle of Least Squares, are the values which make the sum of the squares of the residual errors (or the differences between the observed and computed velocities) a minimum.

The following is the rule which the Method of Least Squares furnishes for finding the values of A, B and C, which satisfy this condition:—

Deduce a *normal equation* for A, by multiplying each observation equation by the coefficient of A in that equation, and adding the results; deduce also a normal equation for B, by multiplying each

observation equation by the coefficient of B in that equation, and adding the results; also one for C, by multiplying each equation by the coefficient of C, and adding. Thus we shall have three normal equations, each containing three unknown quantities, and the solution of these equations will give us the most probable values of A, B and C.[1]

The coefficient of A in the first of the above observation equations is 0, in the second it is 0·01, in the third it is 0·04, etc. Multiplying then the first observation equation by 0, the second by 0·01, the third by 0·04, etc., we have

$$\begin{aligned}
0{\cdot}032299 &= 0{\cdot}0001\,A + 0{\cdot}001\,B + 0{\cdot}01\,C\\
0{\cdot}130128 &= 0{\cdot}0016\,A + 0{\cdot}008\,B + 0{\cdot}04\,C\\
0{\cdot}293499 &= 0{\cdot}0081\,A + 0{\cdot}027\,B + 0{\cdot}09\,C\\
0{\cdot}520256 &= 0{\cdot}0256\,A + 0{\cdot}064\,B + 0{\cdot}16\,C\\
0{\cdot}807050 &= 0{\cdot}0625\,A + 0{\cdot}125\,B + 0{\cdot}25\,C\\
1{\cdot}145052 &= 0{\cdot}1296\,A + 0{\cdot}216\,B + 0{\cdot}36\,C\\
1{\cdot}532034 &= 0{\cdot}2401\,A + 0{\cdot}343\,B + 0{\cdot}49\,C\\
1{\cdot}958016 &= 0{\cdot}4096\,A + 0{\cdot}512\,B + 0{\cdot}64\,C\\
2{\cdot}410479 &= 0{\cdot}6561\,A + 0{\cdot}729\,B + 0{\cdot}81\,C,
\end{aligned}$$

and the sum of these, or

$$8{\cdot}828813 = 1{\cdot}5333\,A + 2{\cdot}025\,B + 2{\cdot}85\,C,$$

is the normal equation for A.

The coefficients of B in the first observation equation is 0, in the second 0·1, in the third 0·2, etc. Multiplying then the first, second, third, etc., equations by 0, 0·1, 0·2, etc., respectively, we have

$$\begin{aligned}
0{\cdot}32299 &= 0{\cdot}001\,A + 0{\cdot}01\,B + 0{\cdot}1\,C\\
0{\cdot}65064 &= 0{\cdot}008\,A + 0{\cdot}04\,B + 0{\cdot}2\,C\\
0{\cdot}97833 &= 0{\cdot}027\,A + 0{\cdot}09\,B + 0{\cdot}3\,C\\
1{\cdot}30064 &= 0{\cdot}064\,A + 0{\cdot}16\,B + 0{\cdot}4\,C\\
1{\cdot}61410 &= 0{\cdot}125\,A + 0{\cdot}25\,B + 0{\cdot}5\,C\\
1{\cdot}90842 &= 0{\cdot}216\,A + 0{\cdot}36\,B + 0{\cdot}6\,C\\
2{\cdot}18862 &= 0{\cdot}343\,A + 0{\cdot}49\,B + 0{\cdot}7\,C\\
2{\cdot}44752 &= 0{\cdot}512\,A + 0{\cdot}64\,B + 0{\cdot}8\,C\\
2{\cdot}67831 &= 0{\cdot}729\,A + 0{\cdot}81\,B + 0{\cdot}9\,C,
\end{aligned}$$

and these added together, give

$$14{\cdot}08957 = 2{\cdot}025\,A + 2{\cdot}85\,B + 4{\cdot}5\,C,$$

as the normal equation for B.

---

[1] The proof of this rule may be found in all books on the subject. See, for instance, *Elements of the Method of Least Squares* (London and New York, Macmillan & Co., 1877), page 48.

The coefficient of C is unity in each of the observation equations. Multiplying by unity leaves them unchanged, and hence their sum, or

$$31\,7616 = 2{\cdot}85\,A + 4{\cdot}5\,B + 10\,C,$$

is the normal equation for C.

The ten observation equations have thus been reduced to the three normal equations:

$$\begin{aligned} 8\,828813 &= 1{\cdot}5333\,A + 2{\cdot}025\,B + 2{\cdot}85\,C \\ 14{\cdot}089570 &= 2{\cdot}0250\,A + 2{\cdot}850\,B + 4{\cdot}50\,C \\ 31{\cdot}761600 &= 2{\cdot}8500\,A + 4{\cdot}500\,B + 10{\cdot}00\,C, \end{aligned}$$

and solving these by any algebraic method, we obtain

$$A = -0{\cdot}7653, \quad B = +0{\cdot}44253, \quad C = +3{\cdot}19513,$$

and these are the most probable and best values. Inserting them in the general parabolic equation for the velocity,

$$V = Ad^2 + Bd + C,$$

we have

$$V = -0{\cdot}7653\,d^2 + 0{\cdot}44253\,d + 3{\cdot}19513,$$

and this is the equation of the parabola with horizontal axis, which agrees best with the above observations. The following table shows a comparison of observed velocities with those computed from this formula:

TABLE II.

Depth of float below surface.	Corresponding value of d.	Observed velocity.	Computed velocity.	Difference.
		FEET.	FEET.	FEET.
Surface.	0·0	3·1950	3·1951	−0·0001
0·1 D	0·1	3·2299	3·2317	−0·0018
0·2 D	0·2	3·2532	3·2530	+0·0002
0·3 D	0·3	3·2611	3·2590	+0·0021
0·4 D	0·4	3·2516	3·2497	+0·0019
0·5 D	0·5	3·2282	3·2251	+0·0031
0·6 D	0 6	3·1807	3·1851	−0·0044
0 7 D	0·7	3·1266	3·1299	−0·0033
0·8 D	0·8	3·0594	3·0594	0·0000
0·9 D	0·9	2·9759	2·9735	+0·0024
Bottom.	1·0		2·8724	

The dotted curve in the above diagram has been plotted from these computed velocities, and it will be noticed that it corresponds much closer to the observed points than that deduced by Humphreys and Abbot. The precision of the two curves may be compared by means of the sums of the squares of the differences between the observed

and computed velocities. Squaring each of the differences in Table I, and taking the sum of these squares, we have 0·00010921; doing the same with the differences in Table II, we have 0·00005693, or only about one-half as much. Hence, according to a principle of the Method of Least Squares, the precision of the dotted curve is to that of the full line curve nearly as 1·4 is to 1. A comparison of the differences in Tables I and II is also interesting—of their signs as well as their values.

In Humphreys and Abbot's parabolic equation,

$$V = -0{\cdot}79222\, d_{11}^{\,2} + 3{\cdot}2611,$$

the origin of co-ordinates is on the axis of the curve at a distance 3·2611 to the left of the vertex, and d_{11} is the distance from that axis in decimal parts of the depth, considered as unity. Hence the equation cannot be used for computing velocities, unless the depth of the axis below the water surface is known. This is stated at 0·297 D (see Remark quoted at beginning of this article). Thus, to find the velocity at a depth of 0·1 D, we must substitute in the equation $d_{11} = 0{\cdot}197$. But in the equation here deduced,

$$V = -0{\cdot}7653\, d^2 + 0{\cdot}44253\, d + 3{\cdot}19513,$$

the origin is on the water surface, and d is the depth below that surface in decimal parts of the whole depth, taken as unity. Velocities may hence be directly computed by substituting for d successive depths below the surface. The position of the axis is, however, readily obtained by dividing the coefficient of d by twice the coefficient of d^2, or

$$d_{11} = \frac{0{\cdot}44253}{1{\cdot}5306} = 0{\cdot}289,$$

and substituting this for d in the equation, we have $V = 3{\cdot}2591$, as the computed maximum velocity. The equation may hence also be written

$$V = -0{\cdot}7653\, d_{11}^{\,2} + 3{\cdot}2591,$$

it being remembered that the axis is 0·289 D below the surface.

The time needed for an ordinary computer to apply the Method of Least Squares to the above problem, is about three or four hours. The process is rapid, it is founded on universally accepted mathematical principles, it has long been recognized and applied in Astronomy, Geodesy and Physics for the determination of such empirical

formulæ, and it gives no chance for any individual choice in drawing the curve.

The parabolic law for velocities at different depths, is the foundation of Humphreys and Abbot's "experimental theory" of the flow of water in rivers, and it is reasonable to suppose that in establishing that law, especial care would have been taken to insure accuracy of calculation, and of statement. That a tedious approximative process should have been chosen for determining the parabola, instead of the strictly scientific Method of Least Squares, is a matter of surprise and regret.

A few errors, which I have discovered in the Mississippi Report, while making the above investigation, may properly be stated here, as they relate to the parabola of vertical velocities. I will explain them by means of the following table:—

Depth of float below surface.	Corresponding value of d_{11}.	Differences between Observed and Computed Velocity.		
		I.	III.	IV.
		FEET.	FEET.	FEET.
Surface.	0·297	+ 0·0049	+ 0·0038	+ 0·0049
0·1 *D*	0·197	+ 0·0006	— 0·0005	+ 0·0006
0·2 *D*	0·097	+ 0·0007	— 0·0004	+ 0·0007
0·3 *D*	0·003	+ 0·0011	0 0000	+ 0·0011
0·4 *D*	0·103	— 0·0009	— 0·0011	0·0000
0·5 *D*	0·203	+ 0·0008	— 0·0003	+ 0·0008
0·6 *D*	0·303	— 0·0066	— 0·0077	— 0·0066
0·7 *D*	0·403	— 0·0047	— 0·0058	— 0·0047
0·8 *D*	0·503	— 0·0002	— 0 0013	— 0·0002
0·9 *D*	0·603	+ 0·0040	+ 0·0029	+ 0·0040

Column I gives the differences from Table I above; these are stated in the Report to have been computed by subtracting from the observed velocities, the velocities given by the equation,

$$V = -0{\cdot}79222\, d_{11}^{\,2} + 3{\cdot}2611,$$

the axis being at a depth 0·297 *D* below the water surface. Inserting in this formula the values of d_{11}, and computing the differences, I was surprised to find that they all disagreed with those in column I. For example, the value of d_{11} for the point 0·1 *D* is 0·197, then $V = -0{\cdot}79222\,(0{\cdot}197)^2 + 3{\cdot}2611 = 3{\cdot}2304$; the observed velocity is 3·2299, hence the difference is — 0·0005. Column III contains

the differences as computed by me from the above equation, and it will be noticed that, with one exception, they are less than those in column I, by the constant quantity 0·0011. Taking then the parabolic equation, whose constant term is 3·2600,

$$V = -0{\cdot}79222\, d_{11}^{\,2} + 3{\cdot}2600,$$

and finding the differences, I obtain column IV, and these all agree with those in I, except that for 0·4 *D* there results 0·0000, instead of — 0·0009.

It hence appears that either the computed velocities and differences, quoted above in Table I from the Mississippi Report, are erroneous, or that the number 3·2611 in the parabolic formula should be 3·2600. Even under this latter supposition there is an error in finding the value for 0·4 *D*. Such blunders—whether due to the carelessness of computer, copyist, or printer—ought not to have occurred in the second edition of a book like this, and least of all at the very foundation of an important theory.

Palæologic Comparative Anatomy.—The lime in egg-shells is not amorphous, but more or less crystalline, presenting many analogies to the shells of mollusks. M. Matheron found in the cutaneous beds near Rognac, two large segments of a sphere in ellipsoid, which he thought to be the fragments of an egg larger than that of the *Aepyorris*, but he hesitated whether to attribute them to a gigantic bird, or to a reptile. M. P. Gervain examined their structure microscopically, comparing them with egg shells of various birds, turtles, crocodiles, and geckoes. The differences of structure are described with interesting detail in his paper, leading him to the conclusion that the eggs resemble those of the Chelonians rather than Crocodilians, while their volume tends to attribute them to the *Hypselosaurus*, which has been thought to be more like the Crocodilians than like the Chelonians. He therefore refers them to a reptile of undetermined classification, analogous to certain *Emyde-saurians*. If that reptile was the *Hypselosaurus*, as there are many reasons for believing, it must have resembled the Chelonians more than the known fragments of its skelcton would lead us to suppose. —*Comptes Rendus.* C.

ON A NEW PROCESS FOR THE ELECTRICAL DEPOSITION OF METALS, AND FOR CONSTRUCTING METAL-COVERED GLASS SPECULA.[1]

By Professor ARTHUR W. WRIGHT, Yale College.

In a paper by the writer, published in this Journal, January, 1877, an account was given of a method of producing metallic films upon the inner surface of exhausted glass tubes, by the action of a succession of energetic electrical discharges. The thickness of these films could be varied, from a tenuity such that the coating barely gave indications of a metallic lustre, and scarcely dimmed the intensity of transmitted light, to the point where perfect opacity was attained, by simply continuing the action of the current for a shorter or longer time. They were produced by forming the negative electrode of the metal to be deposited, exhausting the tube, and passing through it the current from an induction coil. The metallic coatings thus obtained, as seen from the exterior, were very brilliant, but the condition of the inner surface was not readily observed, and the nature of the process made it seem probable that they possessed a dull or even a frosted surface. With a view to obtain the films in a form better suited for examination, a modification of the apparatus was contrived, by which they could be deposited upon pieces of plane glass. At first this object was attained by inserting narrow slips of glass into the tube by the side of the electrode, in the manner suggested in my former paper, and very good results were gained. But, as the nearer portion of the plate received a larger share of the metal, the thickness of the deposit was not uniform, and it was found necessary to construct a special apparatus, in which the relative positions of the plate and the electrode could be varied, so as to give the latter an equal action upon all parts of the surface to be covered. The plan employed was as described in the following paragraphs.

A rather thick-walled glass globe, about seven centimetres in diameter, blown upon the end of a tube twenty-five centimetres long and fifteen millimetres in diameter, was used to form the receiver. The top of the globe opposite the tube was cut off, so as to form an opening forty millimetres in diameter, and the edge ground flat, in a plane perpendicular to the axis of the tube. The end of the latter

[1] From the *American Journal of Science and Arts*, September, 1877.

was drawn somewhat smaller in a gas-flame, and a glass stop-cock attached to it with cement. A little way above this, a platinum wire was fused into the glass to serve as the positive electrode. The cover of the vessel was made by cutting from a similar globe a portion corresponding in size to the part removed, but with the neck attached, the two pieces being carefully ground so as to fit closely. When they were placed together, a little cement applied to the outside along the line of juncture rendered the joint perfectly air-tight. The tube or neck of the cover was five centimetres long, and was also somewhat reduced at the extremity by drawing it smaller. Into this was cemented a small and thick-walled tube extending to a point near the centre of the globe. A platinum wire was placed in this tube, and was fused in at the top, enough being left projecting to form a small loop for the attachment of the wire from the coil. The inner end of the wire terminated at about one centimetre from the lower end of the glass tube. Into the latter was slipped a wire of the metal to be deposited, which, in all cases, was the negative electrode,—the part within the tube being long enough to make good contact with the platinum wire, and being bent somewhat so as to cause it to retain its place by friction. In some of the experiments a different cover was used, made from a glass funnel, the neck of which was left somewhat longer to afford more room for the swinging electrode, as described below, and the tube carrying the latter was fitted into the top by grinding so as to make an air-tight joint.

For the support of the plate, a small watch-glass, about three centimetres in diameter, was employed, to one edge of which a thread of glass was fused by a blow-pipe flame, and then bent so as to form a loop by which it could be suspended like the pan of a balance. A small hook of glass was also attached to the side of the thick tube carrying the electrode, and upon this the pan was hung, the loop being so formed as to allow it to swing freely in all directions. The pan, when in place, was about fifteen millimetres below the end of the tube from which the electrode projected, the latter being adjusted to the proper distance by sliding it up or down in its support, as occasion required. By slightly inclining the globe the extremity of the wire could thus be readily brought over any point of the plate. In some of the experiments the plate was stationary, being held in a little tripod of glass threads, or simply laid upon the bottom of the globe. In these cases the tube holding the electrode was jointed near the

top, the two portions being connected by a hook and loop of platinum or magnesium wire. It could thus be made to traverse all parts of the plate by giving suitable movements to the globe.

When adjusted and closed, the receiver was attached to the Sprengel pump. By means of a small air-pump of the ordinary construction, connected with this by a stop-cock and flexible tube, the whole apparatus was exhausted as far as possible and then dry hydrogen admitted, this being repeated two or three times in order to remove the air and moisture. The process of exhaustion was then completed with the mercury pump. The degree of rarefaction required, varied somewhat with the metal to be deposited, but was rarely above 2·5 millimetres. For platinum the best results were obtained, when it was from 1·5 to 1·75 millimetres. The use of hydrogen is not in all cases necessary, as some of the metals can be deposited perfectly well with only air in the receiver. This is especially the case with gold, but platinum, although ordinarily not easily combined with oxygen, becomes tarnished with a film of what apparently is the blue oxide, unless the air is removed. The electrode itself was formed of a small wire, usually not more than one-fourth of a millimetre in thickness, bent at the end into a circle three or four millimetres in diameter, the plane of which was perpendicular to the straight portion of the wire entering the glass tube, and parallel with the surface of the glass plate situated beneath it. Its distance from the latter was generally about three millimetres, though considerable variations were possible. When it is farther away the process of deposition goes on much more slowly, though the results are in most cases quite as good as when it is nearer. After the process of exhaustion was completed, the stop-cock was closed, and the apparatus removed from the pump, for greater convenience of manipulation in applying the current.

The electrical apparatus employed consisted of an induction-coil capable of giving sparks four or five centimetres in length, and a battery, the power of which could be varied according to circumstances. It consisted usually of pint Grove cells, from three to six in number, not completely filled, or charged with rather weak acid, and a plunge battery of five cells, of which one, two, or more, were used, as occasion required, the whole being joined in a continuous circuit. By immersing the plates of the plunge battery more or less, as well as by varying the number in the circuit, the strength of the current could readily be changed within the limits desired. The various metals

required currents of different strength, and the power best suited to each had to be determined by trial. It was found advisable in most cases to regulate it so that the temperature of the electrode was below that of a red heat, or such as barely to redden it. Of course with the more fusible metals it was necessarily much lower than this. The metal is actually volatilized by the discharge, as is shown by the fact that the characteristic lines of its spectrum may be seen with a spectroscope, and the film is formed by the condensation of its vapor upon the cooler glass surface. For the production of films with brilliant surfaces, the strength of the current must not be great enough to give the discharge a disruptive character, as this separates some of the metal in the form of powder.

The primary object of the experiments was to obtain films of the different metals upon thin pieces of flat glass for the purpose of investigating some of their optical characters. The apparatus proved to be perfectly successful in its operation, and beautiful films of gold, silver, platinum, and bismuth, were obtained with ease and certainty. As has been mentioned, it seemed probable that the surface of deposit would be dull, but the first trial showed that this anticipation was incorrect, and the films, when removed from the receiver, exhibited surfaces of exquisite perfection and the most brilliant polish. They can only be compared to the surface of clean liquid mercury, far surpassing in lustre anything that can be obtained by the ordinary methods of polishing.

This circumstance suggested at once a valuable application of the process in the production of specula for optical purposes, and the subsequent investigations were directed to this end. The mirrors first made had been formed upon disks of thin glass, such as are commonly used as covers for microscopical objects, those being selected which were most free from defects, and had the best surfaces. By means of a very delicate assay balance, the weight of the glass disks, both before and after receiving the deposit, could be obtained to the one-hundredth part of a milligramme, and hence it was easy to calculate the thickness of the metallic layer in any instance. By this means the relative transparency of the different metals can be determined, and the relation between the amount of light transmitted and the thickness of metal traversed by it. The more particular consideration of these and some other matters of interest as bearing upon the optical characteristics of the metals, is deferred to another time, and

it is only necessary to mention here the results of some measurements which were made in order to determine the limiting thickness of a film in regard to the transmission of light; that is, the thickness of a film which would allow only an inconsiderable proportion of the incident rays to pass through. As the metallic lustre is developed gradually with the increasing amount of metal, showing conclusively that light actually penetrates these substances to a certain depth, it was important to ascertain whether the thickness of the layer, sufficient for a virtually complete reflection of light, would be great enough to affect perceptibly the figure of a mirror of glass upon which it was laid down.

Experiments for this purpose were made with gold and platinum, and the process of deposition was continued until the films appeared to have just reached the condition of complete opacity. On removing them from the receiver, however, it was found in both cases that a very small amount of light was still transmitted, as on holding them close to the eye, a brilliant object, like the sun or a bright flame, could be seen through them. The thickness of the gold layer was found to be 0·000183 mm., that of the platinum 0·000174 mm., or approximately one-fourth the length of a wave of light at the red end of the spectrum. The gold, although thicker than the platinum, transmits perceptibly more light, showing that it is the more transparent of the two metals. As the films employed for mirrors may be much thinner than the amount mentioned without an appreciable diminution of the intensity of reflected light, it is evident that the figure of a perfectly wrought glass mirror will not be changed, when the metal is uniformly deposited, to such an extent as to affect its performance unfavorably. A platinum film of one-fifth the thickness of the one described, forms a brilliant mirror, transmitting but a very small percentage of light. The perfect control of the process obtained by the use of the movable electrode will even make it possible to apply the method of local correction for the improvement of a defective figure, or to parabolize a spherical mirror by depositing the metal in a layer increasing in thickness toward the centre, though, of course, it would be better to avoid a somewhat tedious operation by securing the perfect form of the glass beforehand.

Of the metals that are suitable for the formation of specula, platinum appears to be the most valuable. For while, when well polished, it is but little inferior to silver in reflecting power and freedom from

color, it does not become tarnished by oxidation or the action of sulphurous gases, and when dulled by atmospheric deposits the surface can be cleaned by washing with water or with acids, which is an important advantage. By the method here described it can be deposited upon glass surfaces very easily, and a mirror of the most perfect surface produced at once, without the necessity of a single touch to complete it. Several such mirrors have been made in the course of these experiments, by the use of concave glass lenses, with the most satisfactory results. The metal film adheres strongly to the glass, and when of sufficient thickness appears to be very firm and hard. In mirrors silvered by the ordinary method, trouble is often experienced from the insinuation of moisture between the glass and the metal, resulting finally in the separation of the latter. In those prepared by the new process the adherence of the film is so close as to render such an effect impossible. As a test of this, a small silvered speculum was placed in a beaker of water, where it remained for two weeks, and besides this was wetted and dried repeatedly, without showing the slightest tendency to suffer the penetration of the moisture. Similar results were also obtained with platinum and gold films.

With silver the process likewise succeeds well, but it is more difficult to obtain good surfaces than with gold or platinum. The metal is volatilized with extreme ease by the action of the current, and the energy of the discharges must not be too great. Of several trials made with this metal, the most successful was one in which not only the degree of exhaustion of the receiver was less than had been employed in other cases, being only to three millimetres, but the electrode was more distant from the plate, and the battery weaker. The action proceeded slowly in this instance, but with the result of producing an excellent film. With a stronger current the deposit is rapidly made, and has a fine lustre, but the surface has a yellowish color. This is perhaps partially due to a slight degree of oxidation, but also appears to be owing in part to the deposition of a portion of the metal in the form of fine powder, the vapor of the silver, as it streams from the electrode toward the more distant portions of the plate, becoming partially condensed, and falling on it in minute particles. That such a result would follow from this cause, was shown by some of the experiments in which a rather strong battery was employed. The whole interior surface of the globe was in a short time covered with

the powdered metal, appearing an intense purple where thinnest, and shading gradually to deep blue where thickest, the color being the same by both transmitted and reflected light. The metallic lustre was wanting, though it was readily developed when a portion of the powdery coating, which was easily removed, was rubbed against the surface of the glass with some pressure. The defect was, to a considerable extent, remedied by surrounding the electrode with a small glass tube projecting some three millimetres beyond it, so as to clear the surface of the plate by an interval of only one or two millimetres. This had the effect to cut off the lateral portion of the discharge, and to confine its action to a limited area immediately below the extremity of the wire.

The yellow tarnish is removed with the greatest ease by gently rubbing the surface with soft chamois leather and a little rouge, and the metal is so hard, that, when this operation is performed with care, the polish is not at all, or but very slightly, affected. Even then, however, the metal is not perfectly white, having still a very faint yellow tinge. It is well known that silver is not a perfectly white metal, for light which has undergone repeated reflections from polished surfaces of this metal appears yellow or reddish-yellow, though this color is not perceptible when the light has undergone but a single reflection. But the real cause of the yellowish tint may possibly be found in the very tenuity of the films, which, when prepared in this way, have a beautiful and intense blue color by transmitted light. When not too thick, the amount of blue rays which they suffer to pass, may be sufficient to cause, by their abstraction, a perceptible tinge of yellow, the complementary color, in the reflected rays. If this were really the case, the coloration should grow weaker with an increase of thickness, and disappear when opacity is reached. Some of the results obtained seem to favor this view, and the probability of its correctness is strengthened by the facts related in the next paragraph, but further experiments are needed to decide the question satisfactorily.

One result of the investigation has been to show that the color of the light which has passed through a layer of metal varies somewhat with the thickness of the film. This was known to be the case with gold, and experiment has shown it to be true of platinum and bismuth also. Thus the latter in a very thin film appears a clear bluish-gray, while a much thicker film appears brownish. Platinum in a thin layer

has a grayish tint, which varies, as the film is made thicker, to a peculiar brownish shade, somewhat like that of sepia, passing into brownish-yellow, and finally becoming a deep yellow, even inclining somewhat to orange, in the thickest films obtained. Now this color is almost exactly complementary to that transmitted by silver, and the possibility suggested itself of making a mirror which should be perfectly white by reflected light, by depositing first a thin stratum of silver, and over this another of platinum, the relative thickness of the two being properly regulated by observing the color of the transmitted light. An experiment made with a circular disk of flat glass was perfectly successful, the platinum being readily deposited upon the silver, the yellowish tint of which it entirely removed, producing a white and brilliant reflecting surface. By transmitted light the film, as it was anticipated would be the case, has a pure neutral tint, with no perceptible color of any kind.

The value of such a combination for specula is evident, for though, until careful measurements are made, it cannot be asserted that the absolute reflecting power is increased, the whiteness of the layer, and the protection afforded by having the surface covered with an unalterable metal, are very substantial advantages. In constructing large mirrors it will probably also be found to result in a material saving of time, the silver being so much more rapidly and easily deposited than the platinum. The process can also be used with great advantage for the construction of solar eye-pieces for telescopes, since the compound film can be deposited directly upon the surface of the lens, and made thick enough to reduce the intensity of the light as much as may be desired. An image, nearly or quite colorless, could thus be obtained, and the disturbance of the rays should be less than that produced by the interposition of a dark glass of the ordinary kind.

As has been mentioned, some experiments were made with bismuth, and a mirror of excellent surface was obtained, but the metal is inferior to platinum in brilliancy, and has a decided color. The great facility with which films are obtained with it might recommend its use for mirrors in some cases, but for most purposes other metals are to be preferred. Attempts to produce mirrors of iron and nickel were but partially successful, as it was difficult to prevent tarnishing by oxidation. Some good iron films were obtained, however, which were very brilliant. They were exceedingly hard, and adhered to the glass with such tenacity that at first it seemed as if they had been

fused into it. But when the film was dissolved off by an acid, the glass was found not to have been acted upon at all. A singular characteristic of the iron in this condition is its chemical inertness. Films prepared more than six months ago and freely exposed to the air, which, for a part of the time too, was excessively charged with moisture, have not shown the least alteration. Nitric acid, placed upon one of them for a short time, produced scarcely any effect, and nitro-hydrochloric acid acted upon it with about the same readiness as it does upon platinum. This may be due to the extreme thinness of the film, in consequence of which, even the exterior atoms of the iron, being within the range of the molecular action of the glass, are held by a force tending to oppose and neutralize the attraction of reagents that ordinarily attack the metal energetically.

It is not at all necessary that the object upon which the metal is deposited should be of non-conducting material. This is shown by the fact that the process continues to go on after the glass has become covered with a perfectly continuous layer of metal of considerable thickness. The success of the experiment of covering a silvered glass with platinum is additional evidence of the same fact. In order more fully to test the question whether a deposit could be made upon a solid piece of metal, a small silver coin was placed in the pan under an electrode of gold. It was covered in a few minutes with a beautiful coating of the latter metal, which was found to be very hard and to adhere perfectly, having also, in every respect, its proper color and lustre. At the beginning of the process, while still thin enough to allow light reflected from the silver to pass, it had a greenish color, producing a curious effect.

As an example of the applicability of the process to practical purposes, it may be of interest to mention the results of some experiments in the construction of a small Gregorian telescope, the specula of which were covered with platinum by the method described, and with entire success. The larger mirror has a diameter of a little less than four centimetres, and both this and the smaller one, so far as the nature of the surface is concerned, appear absolutely faultless. As only common lenses were employed in its construction, the performance of the instrument is not remarkable, but it is sufficiently good to warrant the assurance that the method will be serviceable for the production of specula of exquisite quality for optical purposes. The size of the apparatus, which, for convenience in experimenting, was neces-

sarily small, did not permit the introduction of larger mirrors than this, but there seems to be no reason for doubting that much larger specula can be successfully made in this way. The amount of time required for obtaining the platinum covering of this mirror was about three hours, during which the coil was kept in continuous action, with a battery power equivalent to four or five small Grove cells. Mirrors of larger size would of course require a longer time, but with suitable apparatus a much stronger battery and larger coil could be used, which would materially accelerate the operation. A plate two centimetres in diameter can be covered with platinum in twenty or thirty minutes sufficiently thick to form a good speculum. For gold or silver the time would not be more than from ten to fifteen minutes.

Many useful applications of this process may be found, and its use is not limited to those metals which have been mentioned here. Moreover, for many of them no other available process is known by which they can be deposited in a uniform layer and with a brilliant reflecting surface upon glass. A very thin layer of platinum, or, still better, of silver and platinum together, could be used with great advantage in the *camera lucida* and similar instruments. Very perfect mirrors for galvanometer needles, and for delicate torsion apparatus, can be expeditiously formed in this way, and by the use of very thin glass, or the most delicate films of mica, they may be made of almost inappreciable weight. For the mirrors of heliostats, and other reflecting instruments in which a metallic surface is necessary, the specula produced by this method will be especially valuable. For telescopes, the beautiful process of Liebig and Foucault, for forming silvered glass specula, is recommended by the ease with which it is applied, and the rapidity of its operation. But the perishable nature of the delicate silver film, and the difficulty of securing a firm and permanent adherence, are serious disadvantages. These are entirely avoided by the use of an unalterable metal like platinum; and though for instruments of the largest size the process here described may be found impracticable, for those of more moderate dimensions there is every reason for believing it may be employed with complete success. The labor and time required for its application are indeed drawbacks; but there is compensation for this in the important circumstance that the mirror comes out of the receiver with a surface of inimitable perfection, which would, in fact, only be injured by any of the ordinary methods of polishing.

ON A NEW TYPE OF STEAM ENGINE, THEORETICALLY CAPABLE OF UTILIZING THE FULL MECHANICAL EQUIVALENT OF HEAT-ENERGY, AND ON SOME POINTS IN THEORY INDICATING ITS PRACTICABILITY.

Presented at the Nashville meeting of the American Association for the Advancement of Science, 1877.

By Prof. ROBERT H. THURSTON, Vice-President.

I.—It is easy to show that the existing common type of steam engine, even if perfect as a piece of mechanism, necessarily wastes a very large proportion of the heat-energy which is supplied to it, and that no possible improvement short of a complete change of type can greatly increase the efficiency of the best modern engine.

A steam engine, theoretically capable of fully utilizing the heat-energy supplied, and of delivering the mechanical equivalent of that heat, has never yet been constructed. The possibility of constructing such an engine has been denied by both physicists and engineers. Nevertheless, theoretically perfect air and gas engines have been designed and built, and a steam engine can probably be made, which may fully utilize all heat not lost by conduction, radiation, and friction. The object of the present paper is to show, not only the possibility of designing such an engine, using steam as the working fluid, but also the probable practicability of constructing a machine which shall waste no heat, except by conduction and radiation, and no power except by friction, under conditions which the engineer will admit to be attainable.

The theory of the proposed new type of engine is perfectly simple; and its construction, although involving the overcoming of grave difficulties in the reduction of losses by friction to a satisfactory extent, may possibly prove no more difficult a problem, than have been many others already solved.

The working fluid is assumed to be steam, because the proposed type of engine cannot be adapted for use with the permanent gases without special and undesirable modifications.

II.—A very simple conception, originating with Sadi Carnot, who published it in his well known work on Heat, half a century ago,[1] has

[1] *Reflexions sur la Puissance du Feu;* Paris, 1824.

furnished a basis upon which has been established the test of a perfect heat-engine. In applying this test, it is necessary to follow the operation of the engine through a complete cycle. The cycle embraces a succession of changes, which concludes by the return of the system to the precise set of conditions with which the cycle commenced. Such a cycle is a *closed curve of conditions*. It is only possible to infer the real relation existing between the heat-energy imparted to any machine and the mechanical energy developed from it after the working fluid, used as a conveyor of the heat-energy in that machine, has been restored, after experiencing a complete cycle of changes, to its primitive condition. The heat-cycle usually corresponds to a kinematic cycle in the engine itself.

During each complete cycle, a certain amount of heat-energy is supplied to the machine, and a certain other amount of mechanical work is done by the machine upon external objects and upon its own moving parts. The result is the accomplishment of a certain amount of useful work, and the consumption, or, more properly, the conversion, of a definite amount of heat in doing that work. Let these two quantities be called A and A'.

Imagine the engine reversed and to complete a cycle in the opposite direction. If the engine be "perfect," every operation is, during this inverted cycle, given a negative direction, and the net result is the re-conversion of the heat A', from the equivalent mechanical energy, A, which is expended in driving the engine backward. An engine capable of reversion in this manner, and with such a result, is a perfect engine and yields the greatest possible amount of mechanical energy from the heat supplied to it.

For, if not, suppose it coupled to an engine which is more nearly perfect, *i. e.*, requires less heat to do a given amount of work. Then suppose the first driven backward by the second. The former restores heat to the source from which the latter receives it. The amount restored is the full mechanical equivalent of the work expended in reversing the assumed imperfect engine. But the heat required by the more perfect machine, which does that work, is less than that amount, and it consequently follows that there must be an accumulation of heat in the reservoir, which heat must have been the product of actual creation—a *reductio ad absurdum*. It follows, therefore, that the reversible engine is a perfect heat-engine, and will yield the full mechanical equivalent of all heat which it is theoretically capable of utilizing.

It follows from the principle of the "conservation of energy," that if a certain quantity of energy is communicated to a system at the commencement of, or during, a cycle, no part of that energy can be extinguished. It must all reappear during the cycle in some form, before the completion of the cycle. In the heat engine there is no known way in which heat-energy can be expended, except by the production of mechanical energy, by loss in friction, and by dispersion by conduction and radiation from the system. In all heat engines, any heat not disposed of in these ways will reappear in its original form of heat-motion.

A perfect engine, therefore, in the sense in which the term is here used, is one in which no heat is lost by conduction, radiation or friction, and in which all heat not discharged unused at the completion of the cycle is utilized by conversion into mechanical energy which may be usefully applied.

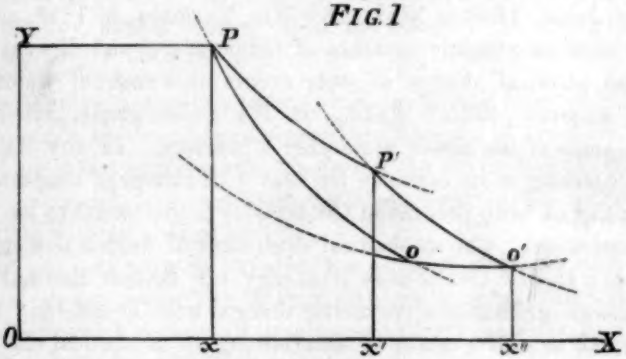
Assuming those conditions which are known to be invariable where the permanent or perfect gases are used as working fluids, to be equally invariable with all working fluids, it is easy to determine, by the application of the principle of Carnot, what is the efficiency of a perfect engine working under any given set of conditions as to temperature. Carnot's conception was, to a certain extent, faulty, in consequence of the fact that, in his time, the character of heat-motion was not understood, the doctrine of the persistence of energy had not been taught, and the science of thermo-dynamics had not taken shape. Sir Wm. Thomson, in 1849, modified the conception of Carnot in accordance with the now well known laws of thermo-dynamics, and gave us the perfect test of the perfect engine.

III.—In illustration, conceive a working cylinder and piston, made of some absolutely non-conducting material. Let the variation of pressure and volume of the working fluid in this cylinder during a single cycle—a double stroke of the piston—on one side of the piston be represented in the accompanying diagram, as in the indicator diagram obtained by Watt from actual steam engines at the end of the 18th century, and as in the theoretical diagrams given by Clapeyron, in 1834. This is such a diagram as would be produced by a pencil moving horizontally with the piston, and at the same time having a vertical motion, proportional to the magnitude of the pressure of the working fluid within the cylinder.

Thus, in the figure, the length of the stroke of piston is measured on the diagram by the distance from the abscissa of the point P to that of the point o'. The pressure varies from that corresponding to the ordinate of P to that of o'. The area of the figure, $P\,P'\,o'\,o$, is a measure of work done.

Suppose the cylinder to contain a certain volume, $O\,x$, of an expansible fluid at the pressure $P\,x$. Let it expand, driving the piston before it and doing work against a varying resistance. Supply to the fluid a quantity of heat, just sufficient to compensate the loss by transformation into mechanical work. The fluid is thus retained at the original temperature, and the curve of varying pressure will be

FIG. 1



an isothermal curve. From the point P' let the fluid expand without receiving heat from any source; it will now lose heat by transformation into work, and its temperature will fall, the line becoming an adiabatic curve, as such a curve has been named by Rankine. At o', reverse the motion of the piston, and remove the heat due to the compression of the working fluid as rapidly as that heat is produced. The pressure line now becomes $o'\,o$, and is an isothermal curve. Let the removal of the heat of compression cease at the point O, which point is so situated that the final return of the piston to its starting point will bring the pressure line, now again adiabatic, to P, and thus restore the fluid to its original condition, as to both temperature and pressure.

Such a cycle can evidently be reversed, each operation occurring in reverse order, and in the reverse direction, and any engine of which this is a representative cycle, is a perfect engine.

IV.—Carnot showed that the maximum efficiency—the maximum proportion of heat utilized to heat supplied—in the perfect gas engine, when the engine transfers heat from a source having an absolute temperature, t, and discharges unutilized heat at a lower absolute temperature, t', is a fraction equal to the ratio of the range of temperature to the maximum temperature:—

$$E = \frac{t - t'}{t}.$$

Benjamin Thompson, Count Rumford, in 1798, indicated the true nature of heat, and by experiment obtained an approximate measure of its mechanical equivalent, which was corrected by the later experiments of Joule, 1843 to 1849. Sir Wm. Thomson, in 1848, showed that we have an absolute measure of temperature (and of heat also, where no physical change of state occurs) in a scale of which the zero is situated 493·2° Fahr., or 274° Centigrade, below the melting point of ice under atmospheric pressure. In any fluid, no change occurring in its capacity for heat with change of temperature, the quantity of heat present in the mass is proportional to its absolute temperature. The mechanical equivalent of heat is now universally taken as 772 foot-pounds of energy per British thermal unit, and as 424 kilogrammetres per metric thermal unit, or *calorie*. These facts furnish us with a means of determining the theoretical efficiency of engines of perfect types working under specified conditions. All existing heat engines are imperfect in proportion as they waste energy by the conduction and radiation of heat to external bodies, and as they waste mechanical energy by friction; since, on reversal, the energy lost in these directions cannot be gathered up by the machine and restored to the source. The heat engine is also, in a certain sense, imperfect, because it is usually the fact that heat rejected unutilized is all lost, and cannot be restored to the source from whence it was supplied to the engine. In some forms of heat engines, a part of this rejected heat is so restored.

V.—Whenever, as is the case with steam, a working fluid experiences a change of its physical state, which results in a change of its specific heat while in the engine and doing work, such estimates of efficiency become only approximate. To secure an accurate measure of efficiency, it is necessary, if Thomson's absolute scale of temperatures is retained, to construct a special thermometer for

each substance, on which the degree must have a different magnitude for each as measured by the ordinary thermometer.

Carnot's unqualified statement, is not, therefore, correct. It is not true that the efficiency of a heat engine is simply "a function of the two limits of temperature between which the engine works, and not of the nature of the substance employed." This is only true when the specific heat remains constant between those limits, and down to the absolute zero. As the specific heat is not always thus constant, it is a matter of importance to consider the nature of the substance used as a working fluid in any case.

When such changes of physical state occur after the working fluid is rejected, they have no importance, except, as will be seen presently, so far as they have a bearing upon the effect of modifying the type of engine.

VI.—Applying the principles which have now been stated to the determination of the maximum possible efficiency of the most perfect of existing steam engines, and comparing the result with the efficiency actually attained, it is easy to determine to what extent improvement is possible without change of type.

Heat engines may be divided, for present purposes, into three principal classes, according to their disposition of rejected heat:

1. Those which restore all heat rejected from the working cylinder to the reservoir from which it was derived.

2. Those which restore a part of the unutilized heat of the working fluid, discharging the remainder from the system and allowing it to be wasted.

3. Those which waste all heat rejected from the working cylinder.

No existing type of steam engine belongs to the first of the classes specified. Some forms of air and gas engines are theoretically assignable to that class, as, by means of some form of "regenerator," they store up rejected heat and restore it to the succeeding charge of working fluid as it enters the cylinder. Actually, however, these engines cannot perform this part of their task thoroughly, and they are thus really to be catalogued in the second class.

Nearly all heat engines, including the steam engine, are most correctly assignable to the third class. In the steam engine, the rejected heat, on leaving the steam cylinder, is thrown into the condendser or discharged into the atmosphere, and, in either case, is wasted. A small portion is usually saved by supplying the boiler

from the hot-well or from heaters in which it has acquired some increase of temperature by transfer from exhausted steam. In so far as this takes place, they fall into class 2. They will here be considered as belonging to class 3.

All actual steam engines may therefore be considered as belonging to one primary class. They all take steam from a source having a high temperature, degrade the heat thus obtained to a lower temperature, doing work and consuming a definite amount of heat in the production of a definite amount of mechanical energy, and finally discharge unutilized heat into the atmosphere or into a large volume of condensing water, by which it is carried out of the system and thrown away.

VII. The steam engine differs radically from air and gas engines of its own class in one respect; and this difference, although passed over as apparently unimportant by accepted authorities on the theory of the steam engine, has constituted a serious and hitherto unsurmounted difficulty in the process of calculation of the exact efficiency of the steam engine.

With all heat engines in which the working fluid experiences no change of physical state, as, for example, in the air and gas engines, we have

$$\frac{t-t'}{t}=\frac{Q-Q'}{Q},$$

the quantities of heat being proportional to the temperatures on the absolute scale. The same is approximately, if not absolutely, true of superheated and of dry steam. It is true, with sufficient exactness for the purposes of the engineer, of water. The proportion is not retained, however, in the fluid which actuates the steam engine —a mixture of steam and water in which the proportions of the two fluids are difficult of determination, if not absolutely indeterminate, at the commencement of the stroke, and in which the proportions are constantly changing throughout the stroke of the piston.

The varying specific heat of the mixture, and the variation in the amount of work done, might be more satisfactorily estimated were it not for the peculiar methods of waste of heat which are characteristic of the steam engine, the most important of which are the internal condensation and re-evaporation, which will be more fully described presently.

Assuming, however, for the moment, that the steam cylinder is made, as in the typical engine of Carnot, of non-conducting

materials, a given amount of steam, entering it at a temperature and volume such that the work done before expansion commences is equal to $p\ v$, is permitted to expand normally, the curve of pressures following, approximately, a line nearly hyperbolic.

As the specific heat of saturated steam is negative, a portion of the charge is gradually condensed as expansion goes on and work is done, and there is finally discharged from the cylinder a mixture of steam and water, the water bearing a higher proportion to the steam remaining as the work of expansion is greater.

But this mixture of liquid and vapor contains, usually, vastly less heat than an equal weight of steam at the same temperature. In any gas engine, $t : t' = Q : Q'$; but here the proportion does not hold, and we have, for steam,

$$\frac{Q-Q'}{Q} > \frac{t-t'}{t}.$$

Hence, the steam-engine *should be* more economical than the air or gas-engine working between the same limits of temperature. That it is not, can only be due to the cause of loss already adverted to as peculiar to the steam engine; or, more correctly, to heat engines, in which the working fluid changes its physical state within the working limits of temperature and pressure. In consequence of the facts just stated, it is seen that the usually accepted method of estimating economy, as in gas engines, is not correct as applied to the steam engine.

VIII.—Neglecting the inaccuracy arising from the peculiar action of steam in the engine, as just described, we will make an approximate determination of the maximum efficiency of the perfect steam engine of the best existing type, and working between the widest limits of temperature generally and successfully attained.

A marine compound engine of the most successful type in general use, takes steam from the boiler at a pressure of 90 pounds per square inch above a vacuum, and discharges the exhaust steam into a condenser having a temperature of 126° Fahr. (53° Cent.), and at a pressure of about 2 pounds per square inch. The original temperature of the boiler steam was 320° Fahr. (160° Cent.) The efficiency should then be:

$$\frac{320-126}{781{\cdot}2} = \frac{160-53}{434} = 0{\cdot}25 +.$$

It should do $772 \times 0{\cdot}25 = 193$ foot-pounds of work for each British thermal unit of heat supplied to it. Per hour and per horse-power of 1,980,000 foot-pounds, it would demand about 10,000 heat units, which would be supplied by the combustion of a pound of coal, evaporating 10 pounds of steam—a very usual and a very good rate of evaporation for steam boilers. Could all of the heat supplied to the engine be fully utilized, however, it would do its work with one-fourth of this consumption of fuel—on $2\frac{1}{2}$ pounds of steam per hour and per horse-power, using $\frac{1}{4}$ pound of coal. But even this estimated efficiency does not represent the full theoretical economy of an ideal steam engine, since, in consequence of the fact already adverted to, that the specific heat of the mingled steam and water, which forms the working fluid, is continually changing as expansion proceeds; and, therefore, the real efficiency is greater than here estimated. This difference becomes very great with great expansion. We may, therefore, say that the perfect steam engine, working under the assumed conditions, would work on considerably less than 1 pound of coal per hour and per horse-power.

The average performance of the best engines built to-day, to work through the assumed range of temperature and pressure, does not even approximate to the calculated efficiency of the perfect engine. The latest and best type of compound engine, taking steam from the boiler at 75 pounds pressure by gauge, and exhausting its steam at a temperature approximating that assumed in the above calculations, requires, usually, about 20 pounds of steam, or, say, 2 pounds of coal per hour and per horse-power. Good engines as now built, therefore, waste at least one-half the heat which is theoretically *available* for the production of power in the existing type of steam engine.

IX.—Now, attempting to apply the test of Carnot and Thomson, we may detect the defects of the machine, and learn what are the causes of loss of all this wasted power:

Tracing the operations which constitute the cycle of the machine in reverse order and direction, we find that such an inverted cycle must include the collection and restoration of all that heat-energy which has been discharged into the atmosphere or into the condensing water at the end of the stroke of the piston; the collection and restoration of all heat which has been lost by radiation and conduction to surrounding bodies; and the reconversion into available heat of all work lost in friction. All this is manifestly impossible in the

existing type of engine, and it is equally evident that the methods of reducing these losses—which are, to a certain extent, inevitable—by friction, and by conduction and radiation, are well known, and are applicable to engines of every conceivable type.

The sources of loss are, then:

1. Friction.
2. Conduction and radiation of heat.
3. The rejection of unutilized heat in the exhaust steam.

The first two of these methods of loss are comparatively unimportant, and, by proper management, can be reduced to a very insignificant amount. It is the last named cause of loss of work that is to be studied, with a view to the determination of some method of greatly increasing the efficiency of the steam engine.

X.—The rejected heat which is exhausted from the engine at the end of each stroke, consists of two parts:

1. That which, in the example above given, constitutes about three-fourths of the heat supplied, and which, in consequence of the natural distribution of the heat throughout the scale of temperature, is necessarily always lost in this type of engine.

2. That which, by the operation known as condensation and re-evaporation in the steam cylinder, is transferred from the steam chest to the exhaust side, at each stroke, without doing its share of the work of the engine.

The method of the first loss of heat has already been indicated with sufficient clearness. The second loss occurs in a way which has but recently become well understood, and which is still not clearly comprehended by many even professional engineers. It is, however, easily explained.

When steam expands in the steam cylinder, it enters at a comparatively high temperature, and at the end of the stroke, at the lowered pressure, has a considerably decreased temperature. There is therefore a tendency, which sometimes has a marked effect, to produce similar changes in the temperature of the cylinder itself—heating it at the entrance of the steam, and cooling it as expansion progresses. The greater the expansion, the greater is this variation of temperature. With considerable expansion, it becomes very great, and, the metal of the cylinder being affected to some depth, the amount of heat passing back and forth between the steam and the

metal is often a very large proportion of the total heat of the entering steam. When, therefore, saturated steam enters the cylinder, it finds the interior surface of the metal comparatively cool, its temperature having been reduced by contact with the exhaust steam just rejected from it. Condensation, therefore, occurs and continues until a sufficient amount of heat has been transferred from the entering steam to the metal of the cylinder, to restore it to the temperature of the prime steam. Thus it is invariably the fact, when superheating is not practiced, that the steam cylinder, at the instant of closing the steam valve, contains both steam and water. The relative amount of the two fluids varies greatly, but it is usually found, where considerable expansion is adopted, that the weight of water is quite great. After the closing of the induction valve, and as expansion progresses, the water, originally at the temperature of its boiling point, under boiler pressure and that of the entering steam, finds itself under a constantly decreasing pressure. Re-evaporation, commencing with the first decrement of pressure, goes on regularly, until the expansion is brought to an end by the termination of the stroke of piston and the opening of the exhaust valve. During this re-evaporation, the water can only obtain its latent heat of evaporation by robbing the surrounding metallic surfaces, after absorbing from the supernatant steam any slight excess of temperature above that due its pressure, which the latter may have itself taken from the metal of the cylinder during that period of rapidly decreasing temperature and pressure which immediately succeeded the commencement of the expansion. Finally, at the opening of the exhaust valve, a sudden drop of pressure occurs, and the whole mass of mingled steam and water remaining in the cylinder at once expands into the condenser, or into the atmosphere, and the re-evaporation of water, which now occurs in great quantity, makes another draft upon the cylinder, reducing its interior to, approximately, the temperature of the condenser or of the boiling point, under atmospheric pressure. Thus heat is taken into the metal of the cylinder from the "live" steam at the beginning of the stroke, and transferred to the exhaust steam, and thrown away without conversion into work. The heat absorbed in re-evaporation before the exhaust takes place, is partly utilized; but that which is taken up at the opening of the exhaust valve, is wholly wasted.

The process of waste just described is one of the most serious causes of loss of heat in the modern steam engine; in some cases, the loss from this cause exceeds that due to the unavoidable rejection of heat at the lower limit of temperature, which only is taken into account in all accepted theoretical estimates of efficiency. It is this method of waste that prevents the engineer acquiring even an approximation to the estimated gain due to considerable expansion. It is this which places a limit to our expansion of steam, which limit has, as yet, been but little altered by any of the expedients which have been adopted to extend it. It has been found by experience that with steam of 60 or 70 pounds pressure, no gain in efficiency can usually be secured by expanding more than five or six times. Passing this limit, the losses due the wasteful transfer of heat to the exhaust steam increase much more rapidly than the gain due to the increased conversion of heat into work by expansion.[1] The higher the steam pressure and the greater the speed of piston, the less the loss from the operation of this cause. The use of superheated steam also reduces it. When the steam is so far superheated that the mass taken into the cylinder may surrender to the metal all the heat required to warm it up to the temperature due the steam pressure, without itself falling to the temperature of saturation at that pressure, this loss is reduced to a minimum. But the saving is effected at the sacrifice of some theoretical efficiency. Steam jacketing produces its well known benefit by checking the waste due to this condensation and re-evaporation.

The losses by the rejection of heat from the engine without transformation are thus seen to be due to two entirely different causes: the first, which is always a proportion, $\frac{Q'}{Q} \times U$, of the total heat supplied, can evidently only be saved by some radical change of type of engine. The second, which has been diminished, but has never been wholly checked by known expedients, seems very probably to require equally radical treatment to effect its cure. It is perfectly evident, nevertheless, that to secure any great increase in the efficiency of heat engines in which steam is to be used as the working fluid, some change must be made by which these losses must be avoided; it is

[1] In general, the number of times which the volume of steam may be expanded in the high-pressure engine with maximum economy, is not far from $\frac{1}{2}\sqrt{P}$, where P is the pressure in pounds per square inch; it rarely exceeds $0{\cdot}75\sqrt{P}$.

only then by a change of type that the now usual loss of nearly three-fourths the heat by rejection at the lower limit of temperature, and of a very large fraction of the remaining energy, can be, to any considerable extent, prevented. Could a change of type be secured by which all this heat could be utilized, giving a theoretical efficiency of unity, we might expect an actual efficiency of at least one-half, and thus obtain a horse-power by the expenditure of five pounds of steam, and the combustion of a half pound of coal per hour. The best existing type of engine has been seen to demand four times this amount, and it is very common for engines to require ten times as much.[1]

XI.—It now remains to be determined whether there is any way by which these losses of rejected heat can be avoided.

There are two forms of engines of class 1st, in which—were it possible to fully avail ourselves of them—all this waste of energy may be avoided:

Type A. The working fluid, if expanded from the temperature and pressure of the boiler or reservoir quite down to the absolute zero, would have all its heat-energy transformed into mechanical work, and there would be no waste. The efficiency would be perfect.

Type B. All heat rejected from the cylinder unutilized may be gathered up and restored to the boiler, there to serve as a basis upon which to pile a new stock of transformable heat-energy, instead of being, as now, rejected from the system entirely and lost. This done, there could be no loss, as all heat leaving the machine would be transmitted to exterior bodies as mechanical energy. Nothing being lost as heat, the efficiency of the engine would be unity and its economy a maximum.

Forms of steam engines may be conceived in which these methods (of saving heat now wasted) may be applied. Practically, however, it is evident, the first form of these two ideal engines can never be made successfully, since it would require to be made of such immense size that all the power derivable from it would be insufficient to move

---

1 Good engines should not require more than $W = \frac{200}{\sqrt{P}}$, where W = the weight of steam per hour and per horse power; the best practice gives about $W = \frac{150}{\sqrt{P}}$ in large engines with dry steam, high piston speed, and good design, construction and management.

the engine itself. The great expansion of steam necessary to secure the reduction of the mass to the liquid state, even—to say nothing of the probable impossibility of making heat contained in the water available—would compel the adoption of cylinders of such great volume as would prevent the adoption of such engines. All advantage theoretically derivable from their use would be more than neutralized by the immense frictional resistances to which they would be subject, even were it possible to construct them.

It is also evident, at once, that this objection would be met, but in a less degree, with engines of the second of these two forms, and that the difficulty would be diminished by extending the range of working temperatures and pressures, and also that it would be lessened by any modification which should reduce the proportion of rejected heat. It follows at once that the direction in which we should look for improvement is that in which the well known expedients for increasing the economical value of the common type of engine may be adopted, with the addition of the single expedient here described of saving and returning to the reservoir all rejected heat after its amount shall have been reduced to a minimum by those familiar methods.

XII.—It is not at all impossible that, although the first of these newly proposed types of steam engine is unlikely to have practical value, a modification of that type may, at some future time, come into use and effect a considerable saving of the now wasted heat. It seems improbable, yet it cannot be said to be impossible, that the first method may be carried so far as to secure the greater part of the heat now rejected in the form of latent heat, and that thus only the heat rejected in the liquefied steam will be wasted. Experiment has not yet determined fully, either the law of distribution of heat throughout the scale of temperature, in either water or steam, or the method of change of physical state. It cannot, therefore, be said whether the peculiar and irregular changes of condition of the fluid, which are observed within the range of temperature familiar to us, are illustrations of similar, and possibly more marked, irregularities of physical modification of temperatures nearer the absolute zero or under pres sures, which Dr. Andrews, and other physicists, and Perkins, Alban, and others, among engineers, have barely begun to study.

We therefore cannot say what would be the behavior of the fluid in an engine of the first type, and cannot even imagine how far the theory of efficiency of heat engines, as usually accepted, may apply

to the steam engine of the now standard type, when the range of temperature of its working fluid shall have been greatly extended. It does seem probable, however, that the steam engine will be found to have a theoretical efficiency, greater than we have previously supposed; and we hope that the practical difficulties to be overcome in the attempt to realize that maximum economy, will be also found to be less than has been anticipated when they are resolutely attacked.

XIII.—It is evident that an engine of class A can only exist when expansion can be carried to such an extent as to leave the working substance at the absolute zero of temperature and entirely deprived of all actual energy.

But Sir Wm. Thomson has enunciated the principle:

"It is impossible, by means of inanimate material agency, to derive mechanical effect from any portion of matter, by cooling it below the temperature of the coldest of the surrounding objects."[1]

Were this principle universally true, it is evident that no engine could be constructed in which the working fluid could be degraded to an average lower temperature than the mean annual temperature of the earth at the latitude of the place in which the engine should be situated. Clerk Maxwell has pointed out the fact that, by a peculiar conceivable, though impracticable, device, the limit fixed by this principle may be exceeded.[11] It is also easy to conceive a much simpler and more natural set of conditions, as will be presently seen, which will permit the limit to be passed, and, possibly, the practical limit to be set back indefinitely in the direction of the zero of heat-motion.

In truth, the principle, as stated, is not correct, except as it implies the existence of certain conditions which are practically persistent, including the use of a permanent gas as the working fluid, or of a substance which does not change its physical state in such a manner, during the conversion of heat into mechanical work, as to invalidate the asserted law.

The fact is, that the conversion of heat-motion into mechanical energy is not limited theoretically, and *may* not be limited practically, by the temperature of the earth's surface.

[To be continued.]

---

[1] On the Dynamical Theory of Heat, etc., by Wm. Thomson; *Trans. R. S. E.*, March and April, 1851.

[11] *The Unseen Universe*, pages 88–89.

ON THE DEVELOPMENT OF THE CHEMICAL ARTS, DURING THE LAST TEN YEARS.[1]

By Dr. A. W. Hofmann.

From the *Chemical News*.

[Continued from Vol. civ, page 208.]

The hot gases from the pyrites kilns are often used for the concentration of sulphuric acid. In this case the leaden pans are placed above or behind the kilns, or the sulphurous acid from the furnaces is passed into a lead tower filled with hard burnt bricks. The arrangement of pans upon the kilns has the disadvantage that if they leak the escaping acid destroys the furnace. It has repeatedly occurred that in cases of such construction the manufacture of sulphuric acid has had to be suspended at the end of the year, and the pyrites kilns entirely rebuilt. It is preferable to place the pans behind the furnace, and to construct a subsidiary flue connecting the kilns with the chamber, so that if repairs in the pans become requisite, the manufacture of sulphuric acid may still go on without interruption.

A much better utilization of the hot sulphurous acid for the purpose of concentration is effected in Glover's tower, which was first introduced in England, and has been described in full by Lunge.[ii] The so-called Glover's tower consists of a leaden chest 4 to 8 metres in height, and of 6 to 10 square metres in superficial extent. For the preservation of the lead it is lined internally with a layer of stones, and is filled with coarse fragments of sandstone or bricks. These materials must be so selected as to resist the action of hot sulphuric acid. Whilst the hot gases from the kilns enter from beneath, they escape at the top in a cooled condition, and are conducted at once into the chambers. Sulphuric acid of sp. gr. 1·5 flows in constantly from above (either alone or simultaneously with the nitrous acid from the Gay-Lussac tower). It is distributed in the tower, comes in contact with the hot sulphurous acid, and escapes below at the sp. gr. of about 1·7. This admirable arrangement was quickly

---

[1] "Berichte über die Entwickelung der Chemischen Industrie Während des Letzten Jahrzehends."

[ii] Lunge, *Dingl. Pol. Journ.*, cci, 341.

adopted in France and Germany, and has everywhere given satisfactory results. By the direct action of the hot furnace gases upon the sulphuric acid, as it occurs in Glover's tower, a powerful evaporation is effected; the sulphurous vapors arrive cooled into the chambers, where the sulphuric acid which evaporates in the tower is likewise arrested, and as the watery vapor which escapes is also conducted into the chambers, there is an economy of steam. Occasionally it has happened that the Glover's tower has been charged with a material so powerfully attacked by the hot acid that the apparatus has been totally choked up and has ceased to act. Another evil involved in the application of Glover's system consists in the fact that no satisfactory arrangements can be made to arrest the flue-dust, since the gases would be too much cooled. Thus the flue-dust is conveyed into the acid, which thus becomes contaminated with iron. For the production of common salt-cake, to be afterwards converted into soda, for the manufacture of superphosphate, etc., the acid concentrated in Glover's tower is perfectly suitable. But the process is less to be recommended for the preparation of sulphuric acid for sale, or for use in the manufacture of salt-cake for the makers of white glass.

The Glover's tower not merely effects the concentration of the chamber acid, but also the denitrification of the acid from the Gay-Lussac tower as already explained.

The further concentration of the acid from 60° to 66° B. is sometimes carried on in vessels of glass, but more commonly in those of platinum. The author has not met with any accurate statements of the outlay for glass, fuel and labor for concentrating the acid up to 1·840, but, according to communications from English manufacturers, the cost is considerably greater than for platinum vessels.[i]

Attempts have been made to render the process of evaporation in glass retorts continuous, but the results of the platinum apparatus are still more favorable. Scheurer Kestner[ii] estimates the loss of platinum at 2 grms. per ton of sulphuric acid. In a letter to A. W. Hofmann, he gives the details of the loss, and states that accurate observations have been made at Thann during three portions of time. From 1854 to 1856, when the sulphuric acid contained a small quantity of sulphurous acid, 1·92 grms. of platinum were dissolved

[i] Scheurer Kestner, *Comptes Rendus*, lxxiv, 1286.

[ii] Scheurer Kestner, *Comptes Rendus*, lxxiv, 1286.

per ton of sulphuric acid of sp. gr. 1·840. From 1856 to 1862, the chamber acid contained nitrous acid, and 2·52 grms. of platinum were dissolved per ton of acid at 1·840. From 1862 to 1866, sulphurous acid was present, and the amount of platinum dissolved was 1·05 grms. per ton of acid of the same strength.

The chemical works at Hautmont (Department Nord) procured in 1865 a platinum apparatus holding 150 litres and weighing 28,548 grms. In 1870 the apparatus was repaired in Paris, when 7891 grms. of platinum were used, but 6275 grms. of old platinum were allowed for, and the weight of the apparatus, by the addition of 1616 grms., was increased to 30,164 grms. At the end of 1873 the apparatus weighed 28,452 grms., showing a loss of 1712 grms. During nine years' working, 6796 tons of sulphuric acid of 1·840 sp. gr. had been prepared in the apparatus. The loss of platinum was, therefore, 0·252 grm. per ton of sulphuric acid. The apparatus cost 30,588·40 francs, at 1050 francs per kilo. The repairs in 1870 cost 3439·95 francs; together, 34,028·35 francs. The worn-out apparatus was sold at 810 francs per kilo. = 23,046·12 francs. The expenditure was, therefore, 34,028·35 — 23,046·12 = 10,982·23 francs, or per 1000 kilos. sulphuric acid at sp. gr. 1·800, 1·616 francs, or about 1*s.* 3½*d.*

(To be continued.)

Amplitude of Sound-Waves.—Lord Rayleigh has communicated to the Royal Society the results of some ingenious experiments upon sound-waves. When the note was f^{iv}, with a frequency of about 2730, he estimated the amplitude at less than a ten-millionth of a centimetre. He is inclined to think that, on a still night, a sound of this pitch would be audible, with an amplitude of a hundred-millionth of a centimetre.—*Nature.* C.

Sahara.—At a late meeting of the Berlin Geographical Society, a report from Dr. von Barry was read. In his excursion into the Tuarez region of the Western Sahara, he found few grounds to sustain the theory that the desert was formerly the bed of a sea. He inclines to the belief that North Africa has long been free from a covering of water, as no traces of tertiary formations were found, and the sand downs cannot be regarded as proofs of an ancient sea. —*Nature.* C.

REMARKS ON MR. MERRIMAN'S (PH. D.) ARTICLE, ENTITLED: "AN ELEMENTARY DISCUSSION OF THE PRINCIPLE OF LEAST SQUARES."

By CHAS. H. KUMMELL, Assistant Engineer, U. S. Lake Survey, Detroit, Michigan.

The author says, in opening, that a simple and yet perfectly satisfactory proof of the main principle of Least Squares, is still a desideratum. His article being written after he had seen mine, which appeared in the *Analyst*, Vol. III, p. 133, etc., it is to be presumed that he would have improved on my representation of the subject. My paper is very abbreviated, as stated by Mr. Merriman, but is, nevertheless, clear and logical to any careful reader, and gives not a mere glimpse of the theory, but almost everything essential. Mr. Merriman's article contains a number of logical and theoretical blunders, which should not go uncorrected.

1. In the author's definitions, p. 174, I object to his restricting the problem of Least Squares to the very special case of the same quantity being measured several times. This explains the deficient enunciation of the principle of Least Squares, on p. 187, where we read: "The most probable values of quantities, measured several times with equal care, are those which render the sum of the squares of the residual errors a minimum." Here the "quantity," on p. 174, has become "quantities," but in either case "they are measured several times with equal care." This enunciation fits neither to the special case for which the arithmetic mean is the solution, nor to the general case which is solved by normal equations, for in the first case we have *one* quantity measured several times, and in the second a *function* of the unknown quantities measured in a certain stadium of that function at different times. These measured quantities are then generally different, and by far, in most cases, they cannot be measured several times. I refer, on this point, to Chauvenet's Least Squares, p. 486, also Art. 6, p. 477, because I think he handles this subject very correctly and clearly.

2. On p. 183, is given the finite equation:

$$\frac{y - y'}{x - x'} = y \frac{2\,\Delta x - 2\,x}{(m + 2)\ 2\ \Delta x^2 - 2\,x\,\Delta x}.$$

In passing to the limit, he obtains:

$$\text{limit}\,\frac{y-y'}{x-x'} = \frac{-2\,y\,x}{2\,(m+2)\,\Delta x^2}.$$

It seems to me that Mr. Merriman, who only says, on p. 181, that m is very great, should consider the term $2\,(m+2)\,\Delta x^2$ an infinitesimal of the second order, which ought to vanish in comparison with $2\,x\,\Delta x$. If m is infinite, as he seems to intimate when he calls $m+2$ "indefinitely great," then the terms in question are each infinitesimals of the first order, and both should be retained; the 2 should, however, be omitted in comparison with m. Where, then, is the evidence, *in the author's line of arguments*, that his limit value is correct? It is correct, however, if the number 2 is also omitted, and the reasons proper are given in "Price's Calculus," Vol. II, p. 378, and p. 134 of my article. Price, however, commits here a different blunder, viz., by assuming that the elementary error, Δx, becomes $d\,x$ at the limit, instead of $\frac{1}{2}\,d\,x$. On this point Mr. Merriman is correct, but he was anticipated by me.

3. The author also says, on p. 183, that Δx^2 is an indefinitely small quantity of the same kind as x. This must be a misprint, although this supposition seems to be untenable, since I have before me a copy of the article, corrected by the author himself. If I read x^2 instead of x, I cannot yet see the logical connection with the statement, "the product $(m+2)\,\Delta x^2$ is hence a positive constant." The product $(m+2)\,\Delta x^2$, or rather $m\,\Delta x^2$, is, however, a constant, for the following reasons: m is an absolute constant, because the total number of element errors, $+\,\Delta x$ and $-\,\Delta x$, is entirely independent of the observations. It must be infinite, however, in order that the probabilities in the third column of the table, on p. 182, may be continuous. $\Delta x^2 = \frac{1}{4}\,d\,x^2$ is a constant for a particular class of observations, larger for poor observations than for good ones. This explains why, if we assume $h = \dfrac{1}{\Delta x\,\sqrt{m}}$, h is a measure of precision.

4. On p. 185, the author says: "The probability y' of any particular error x' depends upon the constants c and h, and will be the larger the greater those constants are; c and h are, hence, related to the precision of the observations." This is correct if c is excepted, which has nothing at all to do with the precision of the observations;

it is an absolute constant, since it is the constant of integration of the equation,

$$\frac{dy}{y} = -2h^2 x\, dx,$$

and the only condition it has to fulfil is that

$$\sum_{-\infty}^{+\infty}\left[y\right] = \sum_{-\infty}^{+\infty}\left[c\, e^{-h^2x^2}\right] = 1.$$

I have proved in my article, p. 135, that, using Mr. Merriman's notation,

$$c = \sqrt{\frac{2}{m\pi}} \text{ where } m = \infty.$$

Eliminating m by means of the equation $h^2 = \frac{1}{2m\Delta x^2} = \frac{2}{m\,dx^2}$, we obtain,

$$c = \frac{h\,dx}{\sqrt{\pi}};$$

and, consequently,

$$y = \frac{h\,dx}{\sqrt{\pi}}\, e^{-h^2x^2}.$$

To test this we have, as we should,

$$\sum_{-\infty}^{+\infty}\left[y\right] = \sqrt{\frac{h}{\pi}}\int_{-\infty}^{+\infty} dx\, e^{-h^2x^2} = 1.$$

It is important that c should be independent of the precision of the observations, for how could it be admitted that (p. 187) the compound probability of a system, viz.,

$$P = c_1 c_2 c_3 \ldots . e^{-(h_1^2 x_1^2 + h_2^2 x_2^2 + h_3^2 x_3^2 + \ldots .)},$$

is to be a maximum, if

$$h_1^2 x_1^2 + h_2^2 x_2^2 + h_3^2 x_3^2 + \ldots . = \text{a minimum},$$

as long as P is also a function of $c_1, c_2, c_3 \ldots .$, which are, according to Mr. Merriman, related to the precision of the observations. Chauvenet, who follows Gauss's reasoning in his "Theoria Motus," makes a similar hasty assumption. He has

$$P = h_1\, h_2\, h_3 \ldots \pi^{-\frac{1}{2}m}\, e^{-[h^2x^2]} = \text{maximum},$$

if $\quad [h^2x^2] = \text{minimum};$

apparently assuming that h_1, h_2, h_3 . . . are of no consequence if they appear as factors of the power of e.

5. Mr. Merriman considers Hagen's proof the best. Does he mean Hagen's own proof, or all those proofs which start with the same fundamental principle? Now this principle, as Hagen says on page 28 of his book, is due to the celebrated English physicist, Thomas Young, who published it in the Philosophical Transactions for 1819. But, although Hagen started with a correct principle, he did not prove Least Squares. Let us see!

On page 38 he arrives at the expression:

$$y = \frac{1}{\sqrt{n\pi}} e^{-\frac{x^2}{n}}.$$

On page 32 we find $n = \frac{1}{2} v = \infty$,
hence, introducing this value of n, we have

$$y = \frac{1}{\sqrt{\infty \pi}} \times 1 = 0.$$

An absurd equation like this would never be employed to prove Least Squares. Nevertheless, Hagen, ignoring this, apparently establishes the principle. The cause of this absurd result is that Hagen departs several times from the straight course of reasoning. (Compare the queer reasoning at the bottom of page 33, and the incompatible equations: $y' - y = -\frac{2x}{n} y$ and $dy = -\frac{2x}{n} y\, dx$ on page 34.)

Mr. Merriman says in a historical sketch, which appeared in the *Analyst*, March, 1877, that I had given Hagen's proof. Now who would like to be accused of such a thing? If Hagen's proof was consistent, it would have been a compliment to me, as I had never seen Hagen's proof before I wrote my article. Mr. Merriman, however, thinks Hagen's proof the best, and he says he gives Hagen's proof in his article. But to do justice to Mr. Merriman, his proof is not near so bad as Hagen's, for the reason that he follows pretty closely Price's presentation, sometimes also adopting, in a disguised form, my own. I take opportunity to state here, that for the first two pages of my article to equation (11), Price's excellent work, already mentioned, has been my only source of information. I corrected, however, an error already mentioned, and discarded the

unnecessary illustration by a curve, which, I think, has rather contributed to obscure the subject. For the remaining part I am indebted principally to Chauvenet's Least Squares, and Fischer's Geodesy. Whatever is not found in these three authors I claim as original.

Electricity of the Torpedo.—M. Marey was led, by some experiments which he made in 1871, to investigate the relations between the voluntary electric discharge of the torpedo and the voluntary muscular contractions of animals. By the aid of M. Marcel Deprez's electro-magnetic signal, which is able to register more than 600 successive electric currents per second, he found that the torpedo shock is a complex act, formed of successive currents, which follow each other at very short intervals, commonly ranging between $\frac{1}{80}$ and $\frac{1}{140}$ of a second. The effects of gradual fatigue upon the discharges, the varying intensity of the current, the correspondence between primary and induced currents, and the analogy to the series of efforts which produce ordinary muscular contraction, are all illustrated by M. Marey's diagrams.—*C. R.*, Jan. 22. C.

The Cave-Dwellers.—Dr. Mitchell, of Edinburgh, places the cave-man in the bone rather than in the stone age. His weapons were made of bone or horn, and highly finished, while his stone implements were extremely rude. The art faculty and the cranial developments of the cave people, show that they possessed a high capacity for culture.—*Nature.* C.

Objection to the Algerian Sea.—In a letter to M. Daubrée, M. Naudin states his fears that if the shallow Algerian *chotts* are filled with water, they will form an immense pestilential breeding place, worse than the Tuscan Maremma or the Pontine Marshes. The greatest depth in the centre of the basin would not exceed 25 metres, and over most of the surface the water would be very shallow. All the conditions would be favorable to an immense multiplication of organic life, decay and miasm.—*Comptes Rendus.* C.

Shaw's Gunpowder Pile-Driver.—M. Lavoinne gives a detailed account of Shaw's pile-driver, as improved by Prindle, with calculations of the work done: 1, till the moment of explosion; 2, at the instant of explosion; 3, during the ascent of the ram.—*Ann. des Ponts et Ch.* C.

THE UPWARD JETS OF NIAGARA.[1]

By W. H. Barlow, F. R. S.

When visiting Niagara last year, after acting as one of the judges at the Centennial Exhibition at Philadelphia, I observed certain physical effects connected with the Great Falls, to which I desire to draw attention.

1. It was observable that the doors and windows of our hotel, unless tightly closed, were subjected to a jarring movement, the impulses of which varied in time and degree.

The hotel in question is Clifton House, on the Canada side; its southern face being parallel to and nearly opposite the American Falls, from which it is distant about a quarter of a mile, and its south-west corner is not far from being opposite to the mean line of face of the Canada or Horse-Shoe Fall, the distance being somewhat over half a mile.

The windows of the hotel opened on hinges, and if one of them was set slightly opened, and the observer placed himself in such a position as to see the reflections of distant objects on the surface of the glass, the times and varying intensity of the jarring impulses could be clearly seen.

2. On looking at the Falls themselves, and especially at the Horse-Shoe Fall, there appeared from time to time, through the mist which always envelops the lower part of the Falls, jets of water projected suddenly upwards.

These jets frequently rose much above the level of the upper part of the fall. Judging from the known height of the Falls they frequently rose from 10 ft. to 30 ft. above the upper level. They occurred at varying intervals; but very few minutes elapsed without seeing one of greater or less magnitude. It was also observable that they had a characteristic form, somewhat resembling a pine-tree, that is to say, small or pointed at the top, and widening out downwards. They were not formed of a compact mass of water, but had that appearance, which is seen in large fountains, of being composed of lumps of water of various sizes, decreasing in the lower part, until they were lost in the general mist which surrounded the lower part of the Falls.

[1] Paper read in Section G, before the British Association at Plymouth.

The continual recurrence of these jets, and the continual recurrence of the jarring action above referred to, point to the conclusion that both effects are due to one cause, and my object in drawing attention to the subject is to endeavor to suggest the nature of the cause which is producing these effects.

Proceeding to a nearer view of the waters by going beneath the Falls, and looking at and through them, it becomes apparent that the water which flows over the upper rocks in a continuous curved stream, breaks up into masses of greater or less magnitude during its descent, so that air in large quantities gets in and between the falling masses of water. In this intermixing of air and water it may frequently happen that a quantity of air is surrounded and enclosed in a heavy mass of water, and falling in this state with a velocity due to the height of 150 ft. or 160 ft., the contained air would become suddenly and violently compressed on striking the rocks below. The energy of the charge of compressed air thus suddenly generated, would burst through the thinnest layer of its surrounding water, and so constitute a species of explosion, carrying a portion of the water with it. Assuming the weight of water which generated the compression to be greater than that on which the energy of the compressed air operated, the effect would be to project the smaller mass of water with a greater velocity than that due to the original fall. The supposition most consistent with the observed phenomena appears, therefore, to be that the two effects, namely, the jets of water and the jarring action shown on the doors and windows, are both due to the explosions or sudden expansions of air compressed by the falling water as above described. There are several circumstances which appear to favor this supposition.

1. The sudden upward blasts of air accompanied by water, felt by persons when beneath the Falls, which are probably only minor effects of a like action.

2. The jarring motion imparted to the doors and windows appears to have no corresponding effect in the solid ground; from which it may be inferred that the effect is due to concussions conveyed through the air, and not to the tremor of the earth by the weight of the falling water.

3. The characteristic form of the jets, which is similar to that produced by explosions under water, when the conditions are such as to throw the water to a considerable height.

And lastly. The suddenness and energy of the operating force as shown by the jets being frequently projected considerably above the level of the upper water.

The inquiry is one of some interest, and may serve to throw light upon those anomalous effects, which have been observed from time to time, with regard to heavy seas falling on a rocky shore, and the extreme height to which the water is occasionally projected under these conditions.

Two notable instances of this kind have been given by the engineers of the English and Scotch lighthouses.

One at the Bishop lighthouse, where a fog-bell weighing 300 cwt., and fixed at a height of 100 ft. above the sea, was displaced and thrown down to the rocks below.

The other occurred at the North Unst Lighthouse, which is built on a rock whose height is 196 ft. above high water, and presenting an almost perpendicular face to the sea. In this case, during a heavy north-west gale, a quantity of water was projected upwards sufficient to overthrow the boundary walls and force open the door of the house.

With regard to the form of jet produced by a subaqueous explosion near the surface, I had the opportunity, a short time since, of witnessing an experiment made by Professor Abel at the Royal Arsenal at Woolwich. The explosive used was compressed gun-cotton. The jet on this occasion rose to a great height, reminding one of the great Crystal Palace fountain, and it was remarkable from the complete verticality of its centre line of force and from the resemblance in its pine-tree form to the jets of Niagara.

Accuracy of French Tuning-Forks.—A. J. Ellis having attacked the accuracy of the French diapason (*Jour. Soc. of Arts*, May 25; *Nature*, May 31), M. Koenig quotes a complimentary letter of Helmholtz to M. Appeur, in which he expresses his astonishment at the accuracy and constancy of his tonometer. He found for his Paris diapason 435·01 vibrations per second, corresponding within less than $\frac{1}{400}$ of 1 per cent. with the official standard of 435 vibrations. Koenig also refers to the corroborative results obtained by Lipajens, Desains, and Mayer, in charging Ellis with unseemly haste, in disparaging the work of a skilful mechanic who could not expect so unjustifiable an attack.—*Les Mondes.* C.

Cosmical Dust.—The attention of modern physicists has been directed to the particles of dust which are at all times floating in the air. Many of them come from the soil, but some are characterized by a special chemical composition and form, which show that they have reached us from the interplanetary spaces. In 1825, Brandes examined, during a number of successive months, the chemical substances contained in the rain water, near Salzüflen, in Germany. He found various organic and mineral substances, among which oxide of iron was especially noteworthy. In 1851, M. Barral, in a series of analyses of the rain water collected at the Paris observatory, distilled 5·57 litres of water, which gave him a dry, yellowish residuum, weighing 183 milligrammes, a portion of which was insoluble in water, alcohol or ether. Its solution in *aqua regia* gave all the reactions of iron. The idea of attributing these minute particles of iron to a fall of cosmic dust seems first to have occurred to Ehrenberg. After analyzing numerous specimens of dust, which had fallen upon vessels at Malta and in the Indian Ocean, he at first supposed that they were of African origin; but noticing their difference from any of the African sands in color, the great geographical distances which they must have traveled according to his first hypothesis, and the amount of oxide of iron which they contained, he broached the theory that they had fallen from the upper layers of the atmosphere.

This hypothesis was adopted and extended by M. Nordenskjöld. In a letter to M. Daubrée, Sept. 9, 1872, he mentions finding, in a careful analysis of snow, soot-like particles, containing organic matter and minute pellets of metallic iron. Thinking that this dust might possibly have come from the chimneys of Stockholm, he requested his brother, who lived in a remote part of Finland, to collect and send him some snow, in which he found particles of the same description. In 1873 and 1874 he repeated his observations at Spitzbergen, and on the glacier of Inlandis, in the interior of Greenland, finding magnetic iron, oxide of iron, nickel and cobalt, which satisfied him of "the existence of a cosmical dust, falling imperceptibly and continually."

In 1875 and 1876 Gaston Tissandier and E. Young, independently collected dust from the towers of cathedrals and other elevated places, which they subjected to chemical and microscopic examinations. By means of a magnet they discovered small spherical corpuscles with a slight roughness, which made many of them somewhat bottle-shaped,

resembling, in appearance, iron which has been reduced to an impalpable powder, and burnt in a hydrogen flame. Young also collected snow at Montreux, Les Avants, Hospice of St. Bernard, and Chanossal, being careful to avoid the lower layers, which might have absorbed something from the soil, and the upper layers, which might have been stained by vegetable débris. The residues from the evaporation of the several samples were first dissolved in distilled water, which separated the chlorides, then in pure chlorhydric acid which showed no trace of iron. Chemical reagents showed the presence of iron in each of the residues, and there were often irregular particles which were attracted by the magnet. He found none of the characteristic globules, but M. Tissandier found them in the sediment of some snow which his brother collected on the side of Mont Blanc, at the Col-des-Fours, at a height of 2710 metres.

M. Young proposes to continue his observations on a larger scale. He feels justified already in affirming that the interplanetary spaces are not destitute of solid materials, but they contain very minute metallic particles; that those particles, when drawn into our atmosphere, play an important part in the dispersion of light, as Tyndall has shown by his experiments; that they help to explain the luminous trains of bolides and the peculiar spectra of auroræ-boreales, and that these microscopic aerolites, by their daily arrival, must increase the earth's mass, so as to afford an explanation, as M. Ch. Dufour has shown, of the moon's secular acceleration. He closes his paper with the following conclusions:

1. That iron exists in all the dust which has been accumulated, in church towers, by the winds of ages.

2. That this iron, floating in the atmosphere, is trapped in its fall by the snow, in which it is always found.

3. That its globular form indicates that it has been raised to a high temperature.

4. That facts tend to prove its celestial origin.

5. That it plays an important part in the physics of the globe, but that science, in order to fully understand it, should seek to estimate the phenomenon quantitatively, and to study it in its variations.—*Bulletin de la Société Vaudoise*, No. 77. C.

MANNER OF LAUNCHING THE STEAMSHIPS PEKING AND TOKIO.

The steamships *Peking* and *Tokio* were built for the Pacific Mail Steamship Co., by Messrs. John Roach & Son, of New York, and Chester, Pa., and run between San Francisco, U. S., and Yokohama, China. They are iron steamers and were built at the Delaware River Iron Shipbuilding and Engine Works, Chester, Pa., and launched March 18th, 1874.

They are 419 feet over all, 396½ feet on 18 foot water line, 47½ feet beam, and 38 feet 5 inches deep from base line to spar deck at centre, and register 5079$\frac{25}{100}$ tons.

The accompanying plate illustrates the method of launching these steamers, which was very successful.

A, Figs. 1 and 2, represents the "ground ways," which rest on the blocking placed on cap piece of piles *A'*, Figs. 2 and 3, and are 430 feet each, fastened together with bolts, and faced with oak 2¾ inches thick. *B* represents the "Bilge ways," upon which the ship rests after the shores and bilge blocks have been knocked away, and are 325 feet long, fastened together by ropes passing through holes in ends.

B', Figs. 1 and 3, is the "packing," extending 300 feet from end to end; at the "ends" the "packing" is held tightly to plating by chains passing through it and under the keel, as shown at *c*, *c'*, *c''*, *c'''*, *c''''*, Fig. 3. The wedges *d*, Fig. 1, are placed every 2 feet apart, and are used for setting up the packing immediately before knocking away shores and blocking, a few moments preceding launching. The bilge ways are fastened to ground ways at *e*, Figs. 3 and 4, by a piece of oak, 3 inches thick, which, when all is ready, is sawed in two, and the ship moves down the ways. The ways are kept from spreading by struts *f*, *f*, *f*, Figs. 2 and 4, and tied together by tie-rods *g*, Figs. 1 and 4, and held apart by distance pieces *h*, Figs. 1 and 4. The ways under the steamship *Peking* were inclined ⅝ inch in 1 foot to the horizontal, and the ground ways covered with tallow ½ inch thick, over which was laid a slush of tallow and oil (3 parts of tallow to 1 of lard oil). The bilge ways were covered with a thin coat of tallow. The *Peking* passed off the ways at this angle in 38 seconds.

Under the *Tokio* the ways were inclined ¾½ inch in 1 foot to the horizontal, and the same lubricant used. At this angle, the ship passed off the ways in 33 seconds. The steamers, at the time of launching, weighed 2560 tons each.

J. E. W.

PLANETARY MECHANICS.

By Pliny Earle Chase, LL. D.

From the earliest days of philosophy, the natural tendency of the human mind towards unity has awakened a desire to resolve all forms of activity into modifications of a single ultimate force. After Franklin's identification of lightning and electricity, there was a general disposition to regard electricity as the primal energy. The investigations of Rumford, Mayer, Joule, Colding, and their collaborators, led to the establishment of the modern science of thermo-dynamics, and a consequent proneness to enthrone heat, in the place of electricity, as the highest "mode of motion."

The Smithsonian Report, for 1876, contains an interesting and important paper, by William B. Taylor, on "Kinetic Theories of Gravitation." His valuable summary of the various attempts which have been made "to discover an origin or antecedent of gravitation," begins with the endeavor of Dr. Robert Hooke, in 1671, "to trace the cause of gravitative fall to the external action of waves in a surrounding medium." Newton's hypothesis of an æther, as the source of "the electric and magnetic effluvia and the gravitating principle," was propounded in 1676; and, in 1693, he wrote to Dr. Bentley: "Gravity must be caused by an agent acting constantly according to certain laws; but whether this agent be material or immaterial, I have left to the consideration of my readers." Villemot in 1707, John Bernouilli in 1734, Lesage in 1750, Euler in 1760, Young in 1807, Herapath in 1816, Jules Guyot in 1832, Faraday in 1844, Seguin in 1848, Boucheporn in 1849, Lamé in 1852, Waterston in 1858, Challis in 1859, Glennie in 1861, the Kellers in 1863, Tait in 1864, Saigey in 1866, Croll in 1867, Leray in 1869, Boisbaudran in 1869, Guthrie in 1870, Crookes in 1873, are all quoted, in order to show the principal forms of the hypothesis that gravity is due to external impulsion, and the prominent defects of such a hypothesis are clearly pointed out.

There is one great objection to all unitary physical theories, in the very nature of mind. We think only under relations, and all relations require at least two correlates. Newton's third law expresses the physical consequence of such necessary correlation, by saying

that "action and reaction are always equal, and in opposite directions." Laplace concluded[i] that the velocity of gravity is at least 100,000,000 times as great as the velocity of light, and that, therefore, "mathematicians may continue to regard it as infinite." This conclusion implies a necessary dualism, by showing the co-existence of instantaneous and progressive activities, and the consequent necessity for an immaterial, as well as for a material, medium. For, if inertia is a necessary quality of matter—time being required to overcome inertia—an instantaneous physical transmission of force is at variance with fundamental physical laws, and utterly inexplicable on physical grounds.

Gravitation, whatever may be its origin, is a centripetal action or reaction. What is its equal and opposite reaction or action? It should obviously be centrifugal, linear, radial and constant. The only form of such activity, of which we have any evidence, is found in the solar radiations which give us light, heat and actinism; which affect terrestrial electricity and magnetism; and which, as Sir John Herschel says, "are the ultimate source of almost every motion which takes place on the surface of the earth." At first glance, it may seem strange to try to make any comparison between light and gravity, but the following extract from Tyndall's lectures shows that radiating energy is fully as important as absorbing energy:

"Look at the integrated energies of our world—the stored power of our coal fields; our winds and rivers; our fleets, armies and guns. What are they? They are all generated by a portion of the sun's energy, which does not amount to $\frac{1}{2300000000}$ of the whole. This is the entire fraction of the sun's force intercepted by the earth, and we convert but a small fraction of this fraction into mechanical energy. Multiplying all our powers by millions of millions, we do not reach the sun's expenditure. And still, notwithstanding this enormous drain, in the lapse of human history we are unable to detect a diminution of his store."[ii]

The centrifugal activities are generally supposed to be propagated through an indefinitely, but extremely, elastic medium, called the æther. Many investigators, however, think that there is no necessity for imagining such a medium, and it may be granted that we have no positive evidence of its existence. But the modes and results of transmission are such *as if* there were such a medium, and the scien-

[i] *Mécanique Céleste*, X, vii, 22, *g*.

[ii] "Heat as a Mode of Motion." N. Y. ed., 1878, p. 466.

tific accuracy of conclusions will not be vitiated by any reasoning which introduces the laws of elasticity for purposes of illustration, or for facilitating deductions.

In the September number of this JOURNAL it was shown that the principal lines of the solar spectrum, and the distances of planets from the sun, are arranged in accordance with harmonic laws. It was further shown that the masses of the principal planets involve a figurate series, which is also found in the arrangement of the Fraunhofer lines. Such results are simple and familiar consequences of well-known laws, which govern synchronous vibrations in elastic media. In following up their indications, for the purpose of seeing if we can find the equality of action and reaction, we may reasonably suppose that the Sun and Jupiter, which are the two governing masses of our system, should exhibit marked evidences of their controlling influence. If we compare them at Jupiter's perihelion, we find that the product of Jupiter's radius-vector by its mass is 1·0153 times the product of Sun's radius by its mass. If we take $n = 1{\cdot}0153$, and $a = 6 \times {\cdot}0153 = {\cdot}0918$, the harmonic progression $\frac{1}{n+a}, \frac{1}{n+2a}, \frac{1}{n+3a}$, etc., gives us the following nodal and internodal divisors:

	Nodal.	Internodal.		Nodal.	Internodal.
1	1·0000		$n + 6\,a$	1·5661	
		1·0536			1·6350
$n + a$	1·1071		$n + 7\,a$	1·6579	
		1·1530	$n + 8\,a$	1·7497	1·7497
$n + 2\,a$	1·1989		$n + 9\,a$	1·8415	
		1·2448			1·8644
$n + 3\,a$	1·2907		$n + 10\,a$	1·9333	
		1·3366			1·9792
$n + 4\,a$	1·3825		$n + 11\,a$	2·0251	
		1·4284			2·0939
$n + 5\,a$	1·4743		$n + 12\,a$	2·1169	
		1·5202	$n + 13\,a$	2·2087	2·2087

It will be observed that the first six internodal divisors are arithmetical means between the adjacent nodal divisors, and that the others are formed by successive denominator increments of $\frac{5}{4}\,a$.

In a condensing nebula we may naturally look for such relations between density and distance as prevail in elastic media. One of the most important of those relations is involved in all barometric measurements of altitude. When the distance from the seat of maximum pressure varies arithmetically, the density varies either geometrically

or harmonically: geometrically if the central force is constant, harmonically if it varies inversely as the square of the distance.

If we take the wave-length of the Fraunhofer line $A = 761{\cdot}20$, as the common numerator of our linear nodal harmonics, and Neptune's mean distance = 6453 solar radii as the common numerator of our exponential internodal harmonics, we find striking accordances between this theoretical order and the actual order of nature, as shown in the centrifugal interferences of luminous waves, and in the centripetal interferences of nebular condensation. These accordances are grouped in the following table. The "quotients" are obtained by dividing 761·20 by the nodal divisors; the "roots," by extracting such roots of 6453 as are indicated by the internodal exponential denominators:

Denominators.	Quotients.	Observed.	Roots.	Observed.	
1	761·20	*A* 761·20	6453	6453	Neptune.
			4130	4122	Uranus.
$n + a$	687·56	*B* 687·49			
		C 656·67 [1]	2015	2050	Saturn.
	634·92				
			1150	1118	Jupiter.
$n + 3a$	589·76	*D* 589·74			
			708	728	Freia.
	550·60		[570	574	Juno].
		E 527·38 [1]	465	473	Flora.
	516·31	*b* 517·70			
			321	327	Mars.
$n + 6a$	486·05	*F* 486·52			
			214	215	Earth.
	459·13				
	435·05	*G* 431·03 [1]	150	155	Venus.
	413·37				
		H 397·16 [1]	111	110	Ven.-Mer.
$n + 10a$	393·73	*H'* 393·59			
			84	83	Mercury.
	375·88				
			66	64	Mercury, s. p.
	359·58				
	344·64		53	53	Mercury, c. o.

[1] *C*, *E*, *G* and *H*, seem to indicate special planetary reactions. The denominators are: *C* 1·1590, Saturn's mean perihelion 1·1576; *E* 1·4434, Earth's major-axis 1·4468; *G* 1·7660, Venus's secular perihelion 1·7640; *H* 1·9166, Mercury's mean aphelion 1·9139. The average deviation of all the lettered lines, from their theoretical values, is less than $\frac{1}{3}$ of 1 per cent.; the maximum deviation is only ·27 of 1 per cent.

The alternate Fraunhofer lines, *B*, *D*, *F*, *H'*, indicate the most striking harmonies, since they are not only nodal, but also figurately nodal. The intermediate lines show the influence of simple secondary harmonies, in addition to their planetary analogies, as may be seen by the following internodal and observed denominators:

$n + a + \frac{a}{2}$	1·1530	*C* 1·1590
$n + 4\,a + \frac{2\,a}{3}$	1·4437	*E* 1·4434
$n + 5\,a$	1·4743	*b* 1·4702
$n + 8\,a + \frac{a}{6}$	1·7650	*G* 1·7660
$n + 10\,a - \frac{a}{6}$	1·9180	*H* 1·9166

The observed internodal positions represent mean planetary distances, until we pass the orbit of Venus. "Ven.-Mer." is the arithmetical mean between Venus's mean distance (155) and Mercury's secular perihelion (64). "Mercury c. o." is the centre of spherical oscillation of a nebula extending to Mercury's mean distance.

It is evident, therefore, that there is a similarity of law governing the centrifugal and centripetal phenomena. It remains to be seen whether we can find a similarity of energy.

Any constant velocity which is due to the action of a central force varying as $\frac{1}{h^2}$, whether the force be centrifugal or centripetal, may be represented by $\sqrt{f\,h}$; any wave- or dissociating-velocity, by $\sqrt{2\,f\,h}$. If the latter velocity is diminished in any degree by the interposition and resistance of inert material particles or bodies, there will be an immediate tendency to central aggregation. The motion which is imparted in the aggregating process must all be efficient, either in rotation, in maintaining molecular orbits, in overcoming inertia, or in some other form of internal or external activity. The limit towards which all the internal and external energies tend, should be the limit which marks the equality of action and reaction for which we are looking.

Whatever may be the internal cyclical variations of velocity in any system, the tendency of the system to association or dissociation with

any other system will be determined by the average velocity. Radial oscillations through twice the major-axis, are synchronous with circular or elliptic oscillations about the same major-axis. The average radial velocity is, therefore, $\frac{2}{\pi}$ of the synchronous average circular velocity, or $\frac{\sqrt{2}}{\pi}$ of the average dissociating velocity. The velocity of planetary revolution, v_1, varies as $\sqrt{\frac{1}{h}}$; the velocity of rotation, v_2, in a contracting or expanding nucleus, as $\frac{1}{h}$. The average internal centrifugal velocity within the sun's mass, $\frac{2}{\pi} v_2$, and the average dissociative centripetal velocity, $\sqrt{2}\, v_1$, would be equalized when $h_2 = \left(\frac{2}{\pi} v_2 \div \sqrt{2}\, v_1\right)^2 h_1$. If h_1 represents the present solar radius; v_1 the present limiting velocity of possible free revolution (revolution at sun's surface); v_2 the present limiting or maximum velocity of rotation (at same point); the limiting velocity of equal centrifugal and centripetal action and reaction is *the velocity of light.*

If the theory of Boscovich were true, or if all the inertia of the solar mass could be removed, or if the limit of centrifugal and centripetal equality could be in any other way attained, the following average velocities would then all be equalized:

1. The æthereal wave velocity.
2. The solar dissociative velocity.
3. The velocity acquired by infinite fall to sun's surface.
4. The average internal centrifugal velocity.
5. The velocity acquired by actual fall through half-radius.
6. The velocity acquired by virtual fall through radius.
7. The velocity acquired by virtual centrifugal action through radius.
8. The uniform velocity which would give the same amount of radial displacement as the varying radial centrifugal velocity.
9. The uniform velocity which would give the same amount of tangential displacement as the superficial dissociating velocity.
10. The velocity of light.

Weber, Kohlrausch and Maxwell have shown that the electrostatic and electromagnetic forces may also be connected by means of the velocity of light. Maxwell, consequently, regards light as a variety of electromagnetic action. Its velocity is the greatest of which we have any positive knowledge, and there are good reasons for believing that it is the greatest possible velocity of strictly physical propagation. It is true that Wheatstone's famous experiments with a rotating mirror, showed an apparent velocity, for electricity, of 288,000 miles per second. But his experiments have never been repeated, and there must have been electrical induction between proximate coils of the conducting wire, which may very likely have been overlooked, and may, therefore, have vitiated his results. The instantaneous velocity of action, which Laplace supposed to attend gravitation, cannot be regarded as a physical or material velocity, in any sense which can be reconciled with commonly accepted definitions of physics and matter.

We thus find in light, heat, actinism, electricity, electromagnetism, gravitation, association and dissociation, evidences of central force propagated with the velocity of light. Centrifugal action seems predominant in some of these different forms or phases of energy, and centripetal action in others. But there is an undoubted dualism pervading them all, and producing cyclical orbital oscillations, to which, perhaps more than to anything else, they owe the distinctive manifestations which lead us to give them distinctive names. There may always be good reasons, on strictly philosophical grounds, for objecting to the appropriation of either of those names, as a technical designation for the primitive force. But the importance of the eyes as educators, the universal diffusion of light, the relations between light and terrestrial energies, and the fact that the velocity of light is the main evidence of a common pervading energy, justify the use, in popular language, of the term which is applied to solar radiation, when we wish to speak of the common energy. The additional consideration of the immaterial element, in instantaneous gravitating action, lends new significance to the words of the SEER: "And the Spirit of God moved upon the face of the waters. And God said Let there be light: and there was light."

HAVERFORD COLLEGE, *Sept. 10th*, 1877.